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NOTICE!

The Register and Directory is now being compiled for the edition of 1910. We expect to issue it in August or September. Please help us to make it as accurate as possible by sending in NOW, any changes of address that have come to your attention. Don't wait a month or two and then send them in; send them in right away.

EDITORIAL NOTES.

Dr. J. J. Arberry, convicted of the crime of trying to obtain money by false pretense, last January, appealed the case and on July 11th the Appellate Court for the 1st District handed down a decision sustaining the verdict and judgment of the lower court. Judge Dunne, before whom the case was tried, was very careful during its conduct and the Appellate Court seems not to have found any errors that would sustain a reversal. For this we should be very thankful, and also to the attorney for the San Francisco County Medical Society, Mr. Walter Kaufman, who handled the case in a masterly fashion and whose time was almost given to the society for this work. At last the point has been raised and sustained that a physician may testify to certain conditions of an individual as matters of fact and not merely as matters of opinion. The reverse, which is an absurdity, has heretofore been held. In the decision the Court says:

"There is little danger of an honorable, upright physician being held to criminal account for a mistaken diagnosis; but where a dishonest physician has made a wilfully false statement as to a mortal disease solely with the view of obtaining money from the victim, the law should deal severely with such physician. We do not mean to even intimate by what has been said that the law will hold a physician liable criminally for a statement honestly but mistakenly made as to his professional judgment in regard to a disease. But the fact that one is a licensed physician will not be allowed as a cloak to shield him from all responsibility for statements wilfully made with the sordid view of obtaining the money of the unwary."

If more courts would take this same view of the relations of the physician to the public, and of the licentiate to the public, there would be a great deal less quackery rampant and there would be a larger respect for the medical profession in the public mind. Perhaps an era of change has come and quackery will be more persistently discouraged in the future than it has been in the past. We can only hope so—and continue to do what we can to convict the Arberrys of the medical profession.

These are not propitious days for dishonest quackery. In January, J. J. Arberry was convicted, in San Francisco, of the crime of obtaining money by false pretense. In June, Dr.

Herbert T. Thornberg was convicted, in Sacramento, of a similar offense. Arberry's case is now before the Appellate Court on appeal; what will happen in the Thornberg case is as yet uncertain, but probably it will be appealed as all these quacks have a large fund for their protection and defense. Thornberg's is a particularly gratifying case, for he was associated with or employed by the notorious Fer Don, an advertising, unlicensed quack, who has been a pest in this state for years. Over and over again has the effort been made to prosecute this quack; but his victims were shy, and he always has a licentiate with him for protection. This time they both went too far. A Mrs. Beck, the complainant, stated to the Grand Jury that Fer Don had guaranteed to cure her rheumatism for \$200, and that Dr. Thornberg, said to be the partner of Fer Don, treated her. Indictments were returned against these two quacks, and on April 28th Thornberg surrendered himself; Fer Don disappeared and has not been located. Thornberg was tried by a jury in June, and, as already stated, convicted. The greatest difficulty encountered in proceeding against these criminals is found in the timidity of the victims; they dread the publicity that comes from exposure and a trial of the case in court. But possibly, now

that it has been made evident that convictions may be secured, victims of other quacks of this kidney may be apprehended. Certainly it is encouraging to have secured two such convictions in six months. Other matters of great interest are nearing completion in the legal department of the Board of Examiners and developments may be expected before long. In Oakland, a notorious quack, one Bohanon, was recently convicted and fined, and others will be put out of business very soon.

Healing cults rise one after another, some to attain vogue for a little space and to die, some to make a more enduring impression. That they arise and that some of them endure is testimony

THE VALUE OF DRUGS

that they meet some need of modern life. Healing cults, proprietary remedies, patent medicines. In spite of increasing intelligence throughout the land and in spite of well organized opposition from the medical profession, osteopathy, naturopathy and christian science do not recede in influence and the jingling streams of gold flow in ever increasing volume to the coffers of Viavi and Cardui. That these things are true is certain evidence that medicine is not fulfilling all its true functions; that the medical man is not "delivering the goods." In our enthusiasm for the Science of medicine we are apt to forget that the end and aim of our profession is to relieve suffering and to master the art of healing.

John Smith, in distress and therefore unable to feed his children, wants quick relief from pain and rapid return to earning capacity, so he summons a doctor; but from his doctor he wants more than this, he wants insight, an instant appraisal of the particular moral and psychic problems that John Smith's illness brings to him. He demands a medical handicraftsman, one skilled in the art of relief, that art to which scientists working in the laboratory or in the clinic have brought so many new facts that it is to-day twofold more powerful than it was a decade ago.

There is no more curious fact to be noted in medicine than that during the very period of cult expansion and expansion of proprietary and patent remedies, the therapeutic nihilist has arisen and waxed mightily in the profession. He has gained so much influence that a large proportion of our younger men consider it quite the thing to disavow a belief in the efficacy of drugs. They are ill-taught in clinical therapeutics, for this branch has little place in the curricula of our schools, and the result is that after a few months' practice they resort to the wide-spreading blunderbusses the detail man leaves on the office table; that is one result; and the weaning away of patients who flock to support this or that, of the so-called health movements, is another.

Therapeutic nihilism, indeed. Let one who disbelieves in the efficacy of drugs watch beside the bed of a loved one racked and torn with pain and there learn the blessing that a wisely administered injection of morphin may become, or let him see the waterlogged, shapeless body of a father or brother grow shapely and fair again under the magic of digitalis or diuretin, or see a dull, mindless cretin, born to some of his own, by virtue of thyroid extract, become an active, normal child. Let him see these things and a hundred more that drugs can do; let him learn what drugs have done; let him master his pharmacopœia and his clinical therapeutics, and soon health cults and vendors' cures will die out and cease.

L. P.

THE WASSERMAN TEST FOR SYPHILIS IN PRACTICE.

It is generally conceded that the discovery of Wassermann's test for syphilis denotes immense progress in our knowledge of that disease; but as experience accumulates, misgivings arise in regard to the extent to which we may trust the verdicts of the test in practice. Dr. Freudenberg recently expressed before the Berlin Medical Society the state of mind into which he had been led by certain occurrences in his practice and exemplified the necessity for caution in accepting the results of the Wassermann method by relating a case in which he had sent a specimen of the patient's blood to a laboratory designated as B. The answer was returned that the result was positive. Three days later laboratory D reported that the result was negative. After another interval of six days blood was again withdrawn, the quantity divided into four parts and distributed among laboratories B, D, C, and E. B, who had formerly reported positive, now reported "doubtful, probably negative." D, who six days before had reported "negative," now reported "positive," and C and E both announced negative results. The clinical course convinced Freudenberg that the lesion which had been the occasion of these tests was not syphilitic.

Freudenberg's communication elicited from others a recital of their experiences: there were instances of contradictions in the reports, doubts made deeper, confusion confounded, but also startling confirmation of the significance of the method. Isaac, who had tested the Wassermann reaction in over 5000 cases, said that caution was necessary on account of our defective knowledge of the nature of the reaction and also because of the different ingredients employed by investigators. The peculiarity of the reaction was illustrated in the case of a young man who applied for treatment for acute gonorrhea; this young man had never had syphilis, and his blood was to serve as a negative control, but the reaction was positive and continued positive on several repetitions of the examination, al-

though he bore no traces of recent or old syphilis. Finally, three weeks having elapsed, a small ulcer was found in the coronary sulcus, which soon became indurated, whereupon the reaction suddenly turned negative and remained negative until the appearance of the rash, when it again became positive.

Wassermann himself then participated in the discussion and protested against ascribing the conflicting or confusing results to the method of serodiagnosis as such. The numerous hospitals for which the tests were made in the Wassermann laboratory had no complaints to make of fallacious or capricious results. He recognized only one Wassermann reaction, that one which was performed strictly according to his directions and with the reagents recommended by him. He had purposely guarded against making the test too sensitive, contenting himself with positive results in 98% of syphilitic individuals; for if too sensitive it might occasionally have branded a non-syphilitic person as syphilitic. Such a slight inaccuracy in the method was deemed harmless, for there had never been any intention of eliminating the physician himself from the solution of the diagnostic problem.

It seems a well-warranted conclusion that the Wassermann reaction, if properly carried out, is of great service in practice. But the reagents are difficult to procure, if the requirements of the original method are to be satisfied, and the test is complicated. It is incumbent upon the practitioner, before acting blindly on the results reported to him from a laboratory, to inquire into the method employed. He will best inform himself as to the trustworthiness of the answers given to his questions by correlating the laboratory findings with his clinical observations in as many cases as possible. Ultimately the agreements or disagreements must show him where to put his faith.

THE ST. LOUIS MEETING OF THE A. M. A.

That the St. Louis meeting of the A. M. A. was a success, goes without saying, and the fact that the attendance was below that of Chicago, two years ago, or that of some of the Atlantic City meetings does not in the least lessen the success. There is the highest authority for the statement that if a few are gathered together in the right spirit success is in their midst, and here there were many times a few. St. Louis itself is not the best arranged city for such a gathering. The meeting halls are all at a considerable distance from the hotels, necessitating long street-car trips, and the opportunities for getting satisfactory lunches in the neighborhood of the halls are few, but even in spite of this we went to the meetings and no one starved. The weather was propitious. Many had feared a hot wave, but instead we had cool and rainy weather, and those of

us who do not often see good old fashioned thunder storms had one for a special treat. It surcharged the Missouri river and washed out a few bridges, while some cyclones and whirlwinds unroofed a number of buildings and nearly blew down the granite-built state penitentiary, just to show visitors from the earthquake belt that the middle west is not wholly asleep by the banks of her big waterways.

It is impossible for one individual to speak of the work of all the sections—therefore this résumé must be limited to one, or to two at the most. The one is the surgical section, the second is the pathological.

It can be said at once that there was no great paper presented unless it was that of Crile regarding hyper-thyroidism and the treatment of the enlarged thyroid with the symptom complex of Basedow's disease. It is not within the function of this article to review the substance of the papers presented and so this mention must suffice with the advice that the paper be read as soon as it is published, for it offers a new view point of the etiology, pathology and treatment. Rodman's paper on carcinoma of the mamma was notable both because of his insistence on the need of a frozen section diagnosis in each case before operation, and also because he had presented a similar paper at the American Surgical Association a month before. If the matter had not seemed to him of great importance he would not have repeated his advice so soon. Grant's paper on Nerve Anastomosis, Sher's on Abdominal Injuries, Ely's on Tuberculosis of the Adult Knee Joint, Bevan's resurrection of Suppurative Peritonitis, are all worthy of careful study. Finally the special paper of the session, the Oration on Surgery, by Abbe, of New York, gave a very fair and full exposition of what is known about and can be done with the master riddle of modern chemistry, Radium. That Crile should be selected to be chairman and Botanly, secretary, suited all and Los Angeles will have a surgical treat when this, the largest and most catholic surgical association of the United States, meets there next year.

It may not be a malapropos, even at this time, to express the hope that a suitable hall or auditorium shall be available in Los Angeles. Many of the men who have the best things to-day do not show custom in speaking in public rooms of large size, and either speak too low even for those with sharp ears, or else address their remarks to the chairman alone. A proper lectern should be provided, situated at the best point for easy speaking to the audience, so that all may hear, and those who discuss the papers should be in the same or an equally well situated place. It seems a pity, too, that telephone messages for members in the audience should have to be constantly announced on the stage, but if it is a positive necessity, it should be arranged for so that the announcement may be made with the least annoyance of those who are listening to the papers or discussions.

The symposium on Cancer in the section on Pathology and Physiology was interesting and all of the papers should be read. All the viewpoints about cancer were occupied and by such men as Loeb of

Philadelphia, Levin of New York, Tyzzer and Mallory of Boston, and Weil of New York. Mallory's photomicrographs, enlarged on the screen, are marvels of accurate presentations of morphological conditions. Finally the surgeon pathologist, Bloodgood of the Johns Hopkins Hospital, closed the symposium with a paper on the results of surgical treatment of various types of carcinoma.

The whole group of papers will give a good exposition of the cancer question as it is thought of to-day and as it is being studied.

In the same section the experimental work of Eisendrath and Straus, of Chicago, on Experimental Ischemia of the Kidney was of practical value to the surgeon, while the studies of Robertson and Chelsea of Minneapolis, on the unsolved problem of the Bacteriology of Acute Anterior Poliomyelitis is of especial interest because of the pandemic of that disease in the United States during the last ten years.

The surgical section was quite satisfied that one of its prominent members—who might perhaps be called one of the teaching members of the section—should be chosen to become President of the Association. John B. Murphy, of Chicago, merits the honor and as a surgeon is a fit successor of Welch, the pathologist, and Gorgas, the sanitarian. The duties of a president are, no doubt, to a great extent perfunctory, but with the stimulus of this honor it is not unreasonable to expect that President Murphy, as head of the medical profession in America, during his incumbency of the high office, may attack and solve some other hard problems in surgical therapeutics, and add to the measures which must always be called by his name.

EMPYRICISM VS. RATIONALISM IN SURGERY.

The average man is sometimes a trifle disinclined to think for himself. He has a good memory and readily learns, parrot-like, the technique of operations. He thinks of operations by the names of the men who described them, rather than by the principles involved and their relative advantages and disadvantages, and proceeds to perform these operations as *set procedures*. He is afraid to leave the beaten path and dare not allow his mind liberty to scheme an operation to suit the case, but instead must do an "Alexander," a "Bassini" or a "Tuttle." He deadens his mechanical sense and is a copier. He uses just such a drain, some certain suture material or a certain chemical in the dressing, not from a knowledge of the various kinds and a judicious selection, but because some other uses it or that he is accustomed to it. Like the theological student, his armament is built on faith. This, of course, does not apply to our many progressive surgeons, but more to the average man who does surgery.

Just as it is better and more rational in prescribing to compile a prescription to suit the case, based on the action of the individual drugs, than to rely on the mysterious healing effect of some worshiped prescription, so in surgery, it is better to learn the principles and processes in the repair of tissues and the effects of different procedures on the physiology, pathology and repair. Under this guid-

ance the work may be done unhindered by convention and with a surety of result.

If the surgeon views the patient always from the physiological standpoint, the measures possible for mending him will always be apparent. For instance, if in a plethoric obese woman in the Trendelenburg position the pulse should become impalpable, the surgeon instead of simply giving stimulants, will understand that the flabby heart has become gorged and has dilated, and he will immediately put the patient in the head up position to drain the blood from the heart, cause the heart to contract down by both cardiac massage and a hypodermic of digitalin and give artificial respiration. If the patient for operation be moribund from shock, the surgeon who understands the principles involved will probably carry him through the operation successfully. He will increase the blood pressure by bandaging the limb, tipping the patient head down and transfusing additional blood into him. He will then operate quickly to minimize the time of anesthesia and deftly to avoid trauma to the tissues. He will use an anesthetic which does not add to the shock, such as a local one or nitrous oxide and oxygen, and in operating will do as little as possible but just enough and of the right things to allow the patient to heal himself.

A well-balanced judgment is a rare gift and it seems that the one idea man is the more numerous. How often do we see a man putter around in the abdomen for hours, bent on super attention to all details, with a total disregard to duration and amount of trauma and the effect of the anesthetic on the patient. Another may devote all his attention to speed, consequently spilling intestinal contents through the abdomen, allowing excessive hemorrhage and crudely adjusting the tissues. One will use silk ligatures, thinking only of the small knot and forgetful that insoluble ligatures give trouble. Another applies a strong disinfectant to the wound to kill germs and disregards the injury it does to the tissues.

The same operative procedures are done in different parts of the country in many different ways and with different materials, but with much the same results. There are, however, just a few underlying principles that are common to all these methods, and the wise surgeon considers these only and adopts the simplest and most efficient methods for their attainment.

The three ways which seem best suited for furthering the progress of surgery are the following, and each is necessary to supplement the others:

First. Careful attention to the development of diagnosis.

Second. The compilation of the clinical courses of thousands of operative cases into statistics, and from these selecting the procedures that give the best results and adopting them. This is exemplified by the Mayos.

Third. The constant trial of new principles and ideas developed from consideration of physiological principles and the consigning of all unsolved problems or clinical failures to the laboratory to be worked out. This method is typified by the work of Crile.

ORIGINAL ARTICLES

THE ESSENTIALS OF SURGICAL DIAGNOSIS WITH REFERENCE TO FRACTURES.*

By T. W. HUNTINGTON, M. D., San Francisco.

It is obvious that end results associated with fracture treatment give quite too large a percentage of faults, deformities and impaired function. Such conditions take a wide range between moderate shortening, slight angularity and alteration of axial relations; to non-union, positive limp, persistent pain and joint ankylosis of varying extent.

In the conduct of fracture cases, the first exaction is to determine, as accurately as may be, the nature of the lesion and to weigh its importance from the standpoint of immediate requirement. But though the lesion be never so obvious and the necessity of immediate attention be clearly urgent, the attendant is, at once, confronted by the more laborious task of perfecting a diagnosis at the earliest possible moment. Routine steps having been taken to insure the patient's comfort and safety, a painstaking review of every feature of the case becomes a matter for serious consideration. First impressions must be revised, early opinions are to be checked up and perhaps abandoned, while collateral or pre-existing conditions are to be recognized and their full value assigned.

At this juncture, immense advantage is to be derived from a carefully written case history, and it may be said that a clear perspective of a clinical picture cannot be acquired and made available without it. In this undertaking a well-defined scheme should be followed scrupulously. During the early hours of an undertaking a mere skeleton should be formulated; this to be rewritten and fully developed, when convenient. Every collateral detail should be worked out and recorded, and no incident having a bearing upon the case, should be omitted. By this means, opportunity is afforded for the correction of errors of commission, while errors of omission, often the forerunner of disaster, are avoided and the patient's welfare conserved in large measure. Even in injuries of lesser magnitude, a written record is often of great value where later developments pointing to occult conditions become ultimately manifest. As a matter of personal discipline, it will be found that the surgeon, himself, derives an appreciable advantage from the careful study of routine cases from this point of view.

While in certain cases a permanent abnormality is inevitable, being inherent in the given lesion, it is nevertheless beyond question that some of the humiliating incidents springing from broken bones can be modified or entirely obviated, by an accurate

knowledge of initial conditions. Joint fractures, notably those of the elbow and ankle, are clearly in evidence, while obdurate, complicated fractures of long bones are of frequent occurrence.

There is a growing sentiment among surgeons, and to a certain extent among the laity, which stands for anatomical reposition, or a close approximation thereto, as the criterion for fracture treatment. Moreover, emphasis should be laid upon the fact, that in exact ratio as the standard for results becomes elevated, the surgeon's moral and legal responsibility is augmented.

If this doctrine be accepted, the logical exaction is for a diagnosis which is beyond question at the outset, and which can be verified during the process of repair.

Is this a reasonable exaction for the routine work of the average surgeon? If this query be affirmed, it is clearly manifest that the attitude of attendants must conform to the added burden thereby imposed. Any policy that suggests or tolerates time-serving or chance-taking must be abandoned and all the sidelights afforded by official aids or methods of precision must be thrown upon the situation.

The natural inference is that the mere discovery of a broken bone and the recognition of deformity or of various complications is but an introduction to the greater task. The genius of diagnosis lies in such interpretation of the pathology of the lesion as will suggest a rational policy for relief; and it may be added that the best validation of a diagnosis is the nearest possible approach to an ideal result.

In recent years, the X-rays have simplified the work of fracture diagnosis and it is a matter of some little surprise that an aid so nearly infallible is not appealed to more frequently. But more surprising is it, that there are those who still insist that the radiogram is positively misleading and unreliable. It seems probable that this belief springs from inexperience as regards technic or interpretation.

It is generally understood that no X-ray investigation is adequate until opportunity for inspection of pictures through two or more planes is afforded and it is an oft-repeated experience, that a marked displacement of fragments may escape detection in a single plane picture.

An expert radiographer was recently severely criticized because a first radiogram (taken under the surgeon's orders) showed perfect alignment in a Colles' fracture, while a second through the lateral plane, taken a few days later, indicated a startling deformity. An appended statement was to the end that the second picture showed a condition that did not exist.

Correct interpretation of shadow pictures is a matter of considerable moment, occasionally calling for an exercise of artistic discrimination that comes

* Read before the Pacific Association of Railway Surgeons, August, 1909.

only with long training. In complicated fractures, it happens not infrequently that multiple lines are obscured or wholly hidden, and the outlines of fragments are hopelessly confused with joint interspaces. In such a contingency valuable aid is to be derived by the study of a picture of the opposite normal member. For this purpose, most X-ray laboratories maintain a more or less complete collection of well executed radiograms of joint structures, but there are, now and then, problems in fracture diagnosis which remain unsolved after exhausting every available resource including the X-ray. In the presence of the usual signs such as crepitus, abnormal mobility, deformity and overriding, with fracture lines and displaced fragments clearly indicated by the skiagram, repeated and conscientious effort at replacement fail, or after more or less satisfactory reduction, permanent fixation, by conservative means, is found to be impossible.

Two cases illustrating this point have been recently observed. The first was an oblique fracture of the tibia with associated fracture of the fibula. Under ordinary manipulation, the fragments seemed quite readily to resume fairly normal relations, but upon adjustment of splints, the deformity was found to have recurred. Again under ether, a strenuous effort was made to reach a satisfactory solution with a similar result. During a later operation, the cause of the difficulty was discovered, in a small, incarcerated fragment, which hung suspended between the fracture surfaces, over which sliding and displacement were inevitable.

Another case was that of a fracture of the femur in the middle third. Having frequently employed pulley traction to aid in reduction of deformity during operative procedure, I determined to resort to this plan in the hope that it would enable us to avoid resort to the open method. Accordingly, under full anesthesia, pulley traction was applied, as much force being used as seemed safe. During this process, the fragments were manipulated and adjusted and to my mind, replacement seemed assured. The alignment was apparently perfect and through the thick thigh muscles, I could detect no irregularity suggesting overriding. With traction still exerted, anesthesia being continued, an X-ray negative was taken to determine officially the exact status of affairs at that moment. Imagine my surprise at finding later that at no time had replacement been secured. These two experiences, occurring in rapid succession as a part of daily routine, will find manifold duplication in fracture treatment everywhere. They sharply accentuate the fallacy of implicit reliance upon tactile sense or even the X-ray in determining the relation of fragments, or the exact cause of displacement.

In such an exigency, having exhausted every resource, there would appear to be the best of reasons for a resort to a diagnostic incision as in dealing

with many conditions affecting the abdominal viscera.

Speaking for those who are in accord with the idea of operative treatment of recent fractures, this doctrine will meet with no protest and I need only suggest that in this field, the transition from the old to the new is not more abrupt nor more startling than that which is manifest in undertakings which have become a matter of daily routine.

Discussion.

Dr. W. I. Terry, San Francisco: I am in accord with the statements made by Dr. Huntington and I have some photographs which illustrate the reposition of bad fractures by operative measures. The whole story is told in these pictures.

Dr. Frank Rattan, Martinez: It seems to me that this subject will become a very important medico-legal point. Dr. Huntington is a very able surgeon and has every appliance possible to make these operations. When I am called out into the country, however, and have nothing but a jack-knife with which to operate, and am then sued for malpractice, and Dr. Huntington is called in as an expert witness, he will say that I did not do right. I suppose that we ought to send these cases down to Dr. Huntington, but I would like to ask Dr. Huntington what we in the country shall do with our fractures of the thigh.

Dr. Karl Kurtz, Los Angeles: I think that where it is impossible to make the diagnosis with the tactile sense and with the X-ray, it is probably advisable to make the diagnostic incision. I recall a case in Pasadena which came into my office in which the diagnosis had been made of dislocation of the shoulder joint. On examining this case the history stated that the head of the bone had been replaced, and the patient further stated that two or three weeks after the injury he had felt something snap and the physician had told him that he had a second dislocation. I examined and found a shortening of the humerus at least 1". I had an X-ray picture taken which confirmed the diagnosis. There might be a possibility in these injuries of not being able with the ordinary examination to make a diagnosis, and I presume that the diagnostic incision would be of value in such cases. I do not think, however, that it would be a good plan for every surgeon or physician to make a diagnostic incision for the simple reason that they are so apt to get infection.

Dr. Edward T. Dillon, Los Angeles: This paper was particularly interesting to me, as I have a great number of fractures in my work in Los Angeles. The point made of taking both the anterior posterior and lateral views of Colles' fractures is an excellent one. Sometimes one view shows the bones to be perfectly aligned, while another made in a different plane shows them imperfectly so. When good apposition of the fragments cannot be obtained I am sure that surgical interference is indicated.

Dr. T. W. Huntington, closing the discussion: The question asked by Dr. Rattan is certainly a proper one, as it is not within the possibilities that every man who is called upon to treat fractures can be expected to study them with the X-ray or to operate in those cases which seem to require operative treatment. In these instances, I should say that a carefully written record is a surgeon's best protection. If it be not possible for the patient to be transported to a point where operation can justifiably be undertaken, the patient should be made fully aware of this fact, and furthermore, should be told that without operation only a tolerable result could be anticipated. If all this be made a matter of record the surgeon need feel no anxiety as to the future attitude of the patient from a medico-legal standpoint.

EPIDEMIC MENINGITIS IN CALIFORNIA AND ITS TREATMENT WITH FLEXNER'S ANTI-MENINGITIS SERUM.

By PHILIP KING BROWN, M. D., San Francisco.

Little is known of the history of epidemic meningitis before the beginning of the last century. It is regarded generally as a disease that this country is responsible for, as epidemics of it were described early in the 19th century along the Atlantic seaboard, before it was noted in Europe. It has probably existed endemically in this country ever since, breaking out in epidemics, in widely-scattered localities and without traceable cause, at distant periods of time. In its epidemic form it varies in severity from an exceedingly mild trouble unattended by complications, to an acute disorder, beginning with a chill and lasting very few hours before termination by death. The chronic form of the disease may extend over several weeks or many months, usually terminating in some permanent loss of function on the part of the patient, if not in complete incapacity. It may finally end in death. Mild cases occur and pass undiagnosed, or may cause as serious a complication as deafness almost without any other severe symptom. Such cases have been reported by Oppenheim, and I have had one in my experience.

The variety of types was wonderfully well illustrated in an epidemic of four cases occurring in Woodland, Cal., several years ago, where all of the children in one family came down with the disease within two days. One died in 18 hours; one in 4 days; one in 6 weeks, and the fourth in 6 months, in the meantime becoming greatly emaciated, mentally incapacitated, deaf and blind.

The disease has broken out on a number of occasions in barracks, and occurred in San Francisco at the outbreak of the Spanish-American War among the soldiers quartered in tents in San Francisco. It resulted in a high mortality and a wide range of disability among the patients who recovered, including deafness, suppurating joint disturbances, otitis media, facial paralysis, mental stupor, strabismus, and pericarditis. A year ago an epidemic of 9 cases occurred among the marines living in barracks at the Naval Training School on Goat Island, in San Francisco Harbor. A more airy and hygienic surrounding or a healthier life could hardly be imagined.

A search of the records of this state shows that there are no data upon which one can count with certainty as to the first appearance here of the disease. In response to communications to some of the older practitioners in the state, I find that in the southern part of the San Joaquin Valley meningitis occurred in epidemic form as early as 1877. Since that time, records of cases of primary meningitis that have recovered are obtainable from various parts of the state, suggesting the probability that the cases were due to the intracellular diplococcus of Wiechselbaum—since pneumococcic and tubercular meningitis are practically always fatal.

To my definite knowledge, in the past two years there have been epidemics or cases in which the diagnosis was proved bacteriologically in San Francisco, Berkeley, Alameda, Pacheco, Clayton, Irvington, Los Altos, Watsonville, Coalinga, Porterville, Bakersfield, Los Angeles, Woodland, Elk Grove and Roseville.

The tabulation of the deaths from meningitis in this state for the year 1907 is given in the accompanying diagram, and shows that during the months of April and May the deaths from meningitis in Central California were in some cases almost double the average for the year. In no other way could this be accounted for than that meningitis was epidemic in these localities at that time. Under the regime of the present State Board of Health an effort is being made to ascertain in each case the type of meningitis, and especially if it could have been the epidemic form. The Secretary of the Board, Dr. W. F. Snow, allowed me to look over at least one hundred letters from doctors, in reply to his inquiries. Only a very few of these were satisfactory in presenting information upon which one could base the diagnosis of epidemic meningitis with any certainty; and none of the data collected in this way has been used in enumerating the localities in which the disease has existed.

The infecting organism was described in 1887 by Wiechselbaum, and is a diplococcus, decolorized by Gram stain, occurring in tetrads or in short chains, difficult to cultivate and of low vitality. It occurs, like the gonococcus, characteristically within the polymorphonuclear leukocyte. It has been found repeatedly in the secretions of the nose, eyes and bronchi, and has been isolated from the blood. It may occur in the nose of persons in contact with the disease, and has been found even in the secretions of normal individuals not so exposed. It is regarded by Goodwin and Scholly as transmissible through the nasal mucous membranes, and the organism has been found by them in the nasal secretion of 50% of the cases examined during the first two weeks of the disease, and in 10% of the people in contact with these cases.

I am describing the disease as I have seen it in California, and not as the description occurs in textbooks. The reason for this is because it will illustrate the wide range of the symptoms from beginning to end that this disease shows in a small number of cases, and thus emphasize its many-sided appearance and progress. The onset in nearly all the cases in one small epidemic was by chill, followed rapidly by headache, nausea, vomiting, unconsciousness and high fever, death terminating the disease in from 5 hours to 2 or 3 days in 11 successive cases. In most cases, however, the onset was comparatively gradual, the patient noticing a dizziness and headache with general malaise and fever, all of the symptoms growing progressively worse, and followed by vomiting and rigidity, especially of the neck. In a certain small number of the cases there was a distinct eruption on the abdomen and chest, and even on the extremities, looking not unlike the roseola eruption of typhoid. Erythematous eruptions are quite common, and are usu-

STATE BOARD OF HEALTH REPORT.

Death from Meningitis, 1907.

Table 2b.

Geographic Division.	Monthly Average	Jan.	Feb.	Mch.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
The State.....	54.7	52	41	58	82	62	55	57	53	45	48	47	56
Northern California													
Coast counties.....	2.3	2	1	5	1	2	3	5	3	2	1	—	3
Interior counties.....	2.7	2	—	2	8	4	3	3	3	—	3	1	3
Central California													
San Francisco.....	12.2	14	10	8	28	17	12	7	10	15	6	10	10
Other bay counties.....	8.7	8	7	15	11	9	10	9	8	4	13	5	5
Coast counties.....	3.7	5	1	5	6	4	3	5	3	2	1	6	3
Interior counties.....	9.7	7	9	9	17	13	7	7	9	6	7	11	14
Southern California													
Los Angeles.....	12.0	12	11	11	9	11	13	13	13	12	14	11	14
Other counties.....	3.4	2	2	3	2	2	4	8	4	4	3	3	4

ally followed by petechiae of varying size. Herpes, especially of the lips, was a frequent symptom, and in one case in an adult an herpetiform eruption covered nearly the entire body. Tache cerebrale was present uniformly. The pulse is usually slow at the start, but varies greatly, and may be very rapid. Respiration is equally irregular, and seems to be dependent upon the amount of brain pressure. Cheyne-Stokes breathing was noted in only one case, that of a child about a year old into whose spinal canal an amount of serum was injected in excess of the amount of fluid withdrawn. It ceased on the withdrawal of some of the fluid a few minutes later. The fever is characteristically irregular, and may in acute cases return temporarily to normal, giving one false hope of convalescence, only to appear again with increased severity (Chart 5.) In this particular it may resemble the course of syphilitic meningitis.

The mental condition of persons receiving prompt treatment was good from the start, if injected early enough. Headache was frequently relieved early, and did not reappear—this in contradistinction to cases not treated by the serum, where intense and persisting headache is the rule. The Kernig's sign, which was a constantly present symptom, as well as the neck rigidity, were invariably the most persistent symptoms, even in cases that recovered after one or two injections. In a few cases the headache was temporarily increased after the injection of serum, but only in cases where a syringe was used, instead of a funnel and tube.

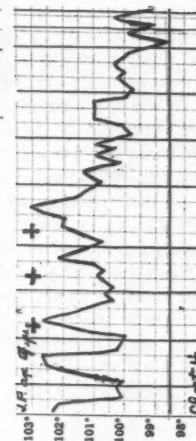
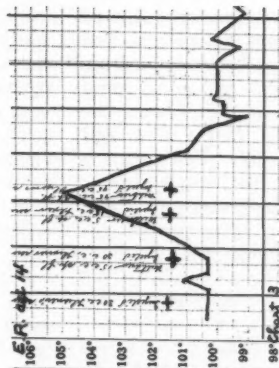
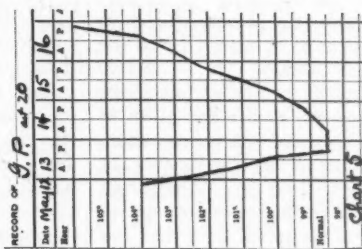
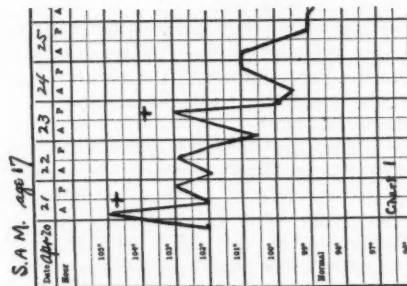
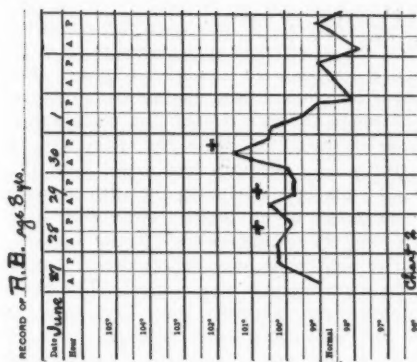
Of the objective symptoms, a polymorphonuclear leukocytosis was constantly present, averaging generally about 25,000. This disappeared very rapidly in cases successfully treated. The character of the fluid removed from the spinal canal is typical of a purulent meningitis, and in the early stages is a slightly turbid fluid, containing large numbers of polymorphonuclear leukocytes and generally easily recognized intracellular, Gram negative diplococci. This rule, however, has very definite exceptions. From one case, ill five days, and unconscious, an apparently clear spinal fluid was withdrawn. It

was considered to be tubercular meningitis, especially as 80% of the leukocytes were mononuclear forms. A few diplococci were found in the fluid, but were not intracellular. The white blood count showed a leukocytosis of 18,000, with 89% polymorphonuclear forms. A further amount of spinal fluid was withdrawn, and on examination showed the definite presence of meningococci. The child was injected with the serum. (Chart 2.)

As tubercular meningitis may be of rather abrupt onset, and may show a polymorphonuclear leukocytosis in the spinal fluid, a definite diagnosis from examination of the spinal fluid is not as easy as might be imagined. In a case of epidemic form seen with Dr. J. O. Hirschfelder, in which the spinal fluid was withdrawn on the fourth day, intracellular diplococci were found only after several hours of search. Had they not been found a positive diagnosis could not have been made. It is nearly impossible to find typical intracellular organisms after the first injection of the anti-meningitis serum. Not only do the organisms almost entirely disappear, but those that remain stain less well and are frequently extracellular. Furthermore, there are very many fewer pus cells. The spinal fluid may become almost clear after the first or second injection.

Cases treated by Flexner serum have shown very few complications; the chief ones seen have been present when the cases were first examined—an optic neuritis and complete deafness in one patient at the end of three weeks of the disease; a facial paralysis in another, and conjunctivitis in a third being among these. This is, of course, a very different matter from the complications one finds in cases treated by other means than the serum—arthritis (often purulent), blindness, deafness, paralysis, imbecility in various forms—being among the commonest of the serious complications seen in cases not treated with the serum, besides inflammations of the mucous and serous membranes which did not occur in any of the cases treated with serum.

To date, the serum has been used on 38 cases, nearly all of which I have seen at least once. There



Apr. 21st.—50 cc. slightly cloudy spinal fluid withdrawn; 45 cc. anti-meningitis serum injected.

Apr. 23rd.—30 cc. spinal fluid withdrawn; 16 cc. anti-meningitis serum injected.

Convalescence uninterrupted.

Chart 2.

June 28th.—5th day of the disease. 60 cc. spinal fluid withdrawn; 30 cc. anti-meningitis serum injected. Boy unconscious; unable to swallow.

June 29th.—25 cc. spinal fluid withdrawn; 45 cc. anti-meningitis serum injected. Boy still unconscious.

June 30th.—Remarkable improvement. Boy conscious; headache gone. 30 cc. spinal fluid withdrawn; 30 cc. anti-meningitis serum injected.

Convalescence uninterrupted.

Chart 3.

On the 7th day of the disease boy unconscious. 30 cc. spinal fluid withdrawn; 30 cc. anti-meningitis serum injected.

The following day 15 cc. withdrawn; uncertain quantity (probably about 15 cc.) anti-meningitis serum injected.

On the next day 5 cc. withdrawn; about 15 cc. injected.

On the next day, under chloroform anesthesia because of the unsuccessful attempts of the two preceding days, 45 cc. spinal fluid were withdrawn, and 45 cc. anti-meningitis serum injected.

have been 12 deaths, or a mortality of about 30%. Of the deaths,

1 occurred in an epidemic where the serum gave out, after decided improvement of the patient from its use.

4 were moribund when injected.

1 got a streptococcus infection of the spinal canal (relapsed case).

1 died of acute diffuse myelitis after 7 injections.

2 were treated entirely by other doctors, with meagre directions, and no complete data of the result was ever furnished me.

Chart 4.

Begins with the 4th day of the disease. At each puncture of the spinal canal (indicated by the crosses), 30 cc. or more of the spinal fluid were withdrawn, and, except at the last injection, 30 cc. anti-meningitis serum injected. An accident prevented more than 20 cc. being injected the last time.

Six weeks later this child had a relapse.

Chart 5.

An untreated case, with the temperature returning to normal on the 2nd and 3rd days of the disease; finally ending fatally.

In the cases that have ended fatally under treatment the temperature has occasionally risen after serum injection, although in most cases it falls. When the injection was made in the earlier cases with the syringe, headache followed not infrequently, as well as pain in the legs. This occurred very rarely after the funnel and rubber tube were used, attached to the end of the needle.

Note. Since writing this article a pamphlet has been issued by Mulford & Co., indicating that they are putting on the market an Anti-Meningitis serum prepared after the method used by Dr. Simon Flexner.

2 of the 9 cases occurring in an epidemic on Goat Island.

(1) fulminant case, after a single injection of 2-3 the present dosage; and

(1) a recurrent case, six weeks after the first attack, where the disease was probably very diffusely spread over the meninges.

There have been 2 relapses in the 38 cases, both of them dying. Neither one was as promptly treated in the relapse as in the first attack.

The technic and the effect of the injections need a special word. As the serum is not an anti-toxin,

but possesses strong bacteriolytic power, and owes its efficiency to this fact, it must be injected into the spinal canal in maximum dosage. It is utterly valueless given subcutaneously. The serum seems to have no bad effect whatever, even when given in doubtful cases before bacteriologic examination. Early in the use of the serum the inefficiency of small doses was shown (see Chart 3). The serum seems to be most efficient if given in amount equivalent to the total amount of the spinal fluid that can be withdrawn. In my experience it is difficult to get more than 50 c. c. of the spinal fluid at one time, even from an adult. The usual dose administered now is three bottles of the serum, or 45 c. c. (see Chart 1). The serum must be at body temperature when injected, and is best put into the spinal canal by attaching to the needle used in withdrawing the spinal fluid, a small rubber tube, to the other end of which is attached a glass funnel. The serum is poured into the funnel before the attachment is made, so that all the air may be gotten out of the tube connection. This has the advantage of enabling the serum to be put in under low pressure, and causes less frequent headache and pain in the legs than where it is put in with a syringe.

Differential Diagnosis.

The difficulty of determining whether one has to do with tubercular or epidemic meningitis has already been referred to. I have had one experience in a case of a doctor's child, in a country town where epidemic meningitis had occurred, where the boy, convalescent from mumps, developed a sudden temperature of 102°, with headache and vomiting. All these symptoms developed within a few hours one evening in a lad of 9 years, apparently perfectly well prior to this except for mumps. The following afternoon, when I saw him, he had marked rigidity of the neck, Kernig's sign, knee reflexes absent in the right leg and almost gone in the left, unconsciousness, and temperature of 103°, with a slow pulse. There was no eruption, and no history of tuberculosis in any of the family. The spinal fluid when withdrawn was slightly turbid—as much so as I should have expected in the first day of the epidemic form of the disease. I gave the boy 45 c. c. of anti-meningitis serum, and took the spinal fluid to town for examination. Nothing grew in the cultures, and no bacteria of any kind were found in the fluid. There was a very striking number of lymphocytes—from 15 to 20 in every field—and hardly more than one polymorphonuclear form to this number of lymphocytes. A positive diagnosis of tubercular meningitis could not be made without the finding of the organism, although every other clinical symptom, including the lymphocytosis, pointed to it. The case would have been additionally interesting had tubercle bacilli been found, because the boy recovered. He was given 60 to 70 grains of urotropin a day, and spinal fluid was withdrawn 24 and 48 hours after the first withdrawal.

It would seem scarcely necessary to discuss any other condition that might be mistaken for meningitis, further than to refer to the extreme frequency in children of meningeal symptoms with the

onset of acute diseases. I have met this condition three or four times, where it was necessary to withhold a positive diagnosis for a number of hours. The spinal fluid from these cases contains no cellular elements of moment, and if such an examination can be made it should assist in the diagnosis. I have seen such a case within a few weeks with Dr. Orella, in a Greek boy, where on the fifth or sixth day after the onset of an acute illness with high temperature, migratory pneumonia became very apparent, and a diphtheria was superimposed before the crisis of the pneumonia was reached. The meningeal symptoms for the first few days, during which time the respirations were 16 to 18, although the temperature was 105°, were the most marked symptoms that the child had.

A case of meningitis from coccidial infection, seen with Dr. Ryfkogel, baffled positive conclusive diagnosis until autopsy, although the nature of the meningitis was suspected. Dr. Ryfkogel will report the case in full later.

Syphilitic meningitis, or acute syphilitic encephalitis, must be considered carefully in certain doubtful cases. A child of 11 years, convalescing from measles (seen with Dr. Jump), developed meningeal symptoms on the eighth day, and sank into coma rapidly. Her temperature reached 103°, and she had strabismus, irregular pupils, absent knee-jerks, Kernig's sign, and rigid neck. The spinal fluid was clear and contained no evidences of tubercular infection in organisms or cell-count. After three days the temperature fell to normal for 36 hours, although the coma continued. In the meantime, the spinal fluid showed a positive Noguchi test, butyric acid test and Nonne reaction. The number of leukocytes was never significant, as is commonly the case in syphilitic meningitis. The fever returned after a 36 hours fall. The child recovered on specific treatment.

Anterior poliomyelitis has occurred epidemically in California. The first epidemic (at La Grande) was reported by Newmark. The second (in and about San Francisco), by Alice Wood. The third (at Watsonville), by me, and also the fourth (at Red Bluff, last fall). Two of the cases from the last epidemic came under my observation, and Dr. John Fife of Red Bluff refers to the epidemic in a letter to the State Board of Health. There is nothing in the accurately-worked-out picture of the acute condition or its complications to suggest meningitis, but I have seen such cases diagnosed as meningitis, and the paralysis attributed to a consequent neuritis. As far as I know, the neuritis of epidemic meningitis is limited to the cranial nerve. The posterior nerve-roots of the spinal cord have been shown by Councilman to be the seat of inflammation, and lymphoid cells are sometimes found among the nerve fibres which show some degeneration; but in no discussion of the disease that I have been able to consult have I found any reference to a complicating neuritis that could be mistaken for the spinal cord changes of anterior poliomyelitis. The neuritis that may occur with the onset of some of the cases of meningitis that are sub-acute from the start, e. g.: double sciatica has been reported, can hardly be so mistaken. In one of my

cases an acute myelitis of the cord occurred, without bacterial invasion. The case was studied by Dr. Ophuls in sections and cultures.

It remains to say only a word about the distribution of the serum. As long as the supply is exceedingly limited, and while its use is regarded as experimental, it is necessary to make the most use of the clinical observations of cases that are treated with the serum. For this reason its use has been confined to such cases as were possible to prove by bacterial examination, and of which an accurate record of the symptoms and course of the disease could be obtained, and where the careful technic recommended by Dr. Flexner could be carried out. I have made efforts to have the State Board of Health take up the manufacture of the serum, on account of the wide prevalence of the disease in California, and it seems possible that in the near future some provision will be made for its production.

TREATMENT OF FRACTURES OF HUMERUS THROUGH MUSCULO-SPIRAL GROOVE.*

By REXWALD BROWN, M. D., Santa Barbara.

Simple fractures of the shaft of the humerus have in the past exacted special consideration in some 5 to 8 per cent of cases, because of involvement of the musculo-spiral nerve to the extent of paralysis, as manifested by inability to extend fingers and hand, loss of power of supination, and impaired sensation in back of forearm. This paralysis may appear coincident with the fracture or as a complication of later days or weeks.

In primary paralysis the nerve may be contused, may be lacerated, may be stretched, impaled on a jagged fragment, pinched between fragments, crushed within its perineurium, or altogether severed.

In the secondary type of paralysis it is almost uniformly found that the nerve suffers from pressure of the bony callus. This callus may form tunnels about the nerve, compressing it at points or for considerable length. Sharp spicules may project into the nerve and partially or wholly sever it—or the callus may lift the nerve trunk from its bed, producing various angulations.

That the operative treatment of simple fractures of the long bones, in the hands of careful surgeons, almost invariably gives perfect anatomical and functional results is not yet fully appreciated by large numbers of the profession. It is impossible to expect that the pioneers in this branch of surgery, practically a work of the last few years, can at once break the inertia of the conservative and expectant methods of handling fractures which have existed for many hundreds of years. The profession has

been taught to believe and has learned to know that certain types of fractures yield almost a definite per cent of ill results regardless of the treatment applied. Surgery, therefore, which discredits these beliefs, these facts, has been too revolutionary to accept.

The above is merely prelude to my conviction that closed fractures of the shaft of the humerus through the musculo-spiral groove are absolute indications for open treatment, not alone that the fractures may be handled in a masterly way, but also that musculo-spiral injuries, when immediately present, may be managed properly at the time, and that such complications, arising during callus formation may be prevented. It is an unhappy occurrence, a patient having been assured that the bone is knitting kindly, to be confronted with a useless arm after the splints are removed—most unhappy for the patient, extremely embarrassing for the doctor.

A great many text-books urge an expectant treatment in the primary type of paralysis, emphasizing the hope that the nerve may be only contused, and that molecular changes will be rectified shortly. At any rate, so the statements read, though the nerve be divided or crushed, a secondary operation in the future will be productive of as good results as would have immediate interference which necessitates opening of the flesh down to the fracture surfaces, seriously jeopardizing bony union. This latter contention scarce carries weight to-day, so overwhelming are the excellent results in fracture repair by the open method.

The nerve concerns us—it is not fair to a patient's best interests to trust to nature to cure a contusion, when we do not know a contusion is present—nor is a contusion to be considered lightly, for combined with minute lacerations, it may be the forerunner of cicatricial deposits in the nerve trunk to interfere with carrying of impulses. From the basis of experience we do know that a lacerated, pinched, stretched or divided nerve is more than probably responsible for the paralysis.

Such conditions are serious lesions. It is impossible that operations conducted at considerable intervals after the damage has been done can give as favorable prognoses as primary procedures. Let us know the reasons: After the partial or complete division or crushing of a nerve trunk conduction ceases in the peripheral segments of the severed axones, and degeneration of the fibres begins at once. There is also degeneration in the central end to the first constriction of Ranvier. Within the usual period the space between the divided and retracted nerve ends becomes filled with cicatricial tissue which often penetrates into, surrounds or constricts either one or both segments. Neuromata and bulb formation occur not rarely to further damage the insulted nerve trunk.

The longer the central cells in the cord and spinal ganglia are deprived of external stimuli, the greater is the disturbance of the trophic energy, and les-

* Read before the Pacific Association of Railway Surgeons, August, 1909.

sened is their power to properly handle impulses when conductivity is again established by anastomosis of the severed ends. Though regeneration of axonal wires in the peripheral segments appears almost simultaneously with the degenerative processes,—arising from the proliferation of the chains of cells, comprising the Sheath of Schwann, or neurilemma—new fibers are imperfectly developed without the establishment of conductivity.

Now, therefore, the surgeon who undertakes musculo-spiral nerve repair some weeks or months after the onset of paralysis finds very often discouraging obstacles to surmount, which lessen the probabilities of complete functional return to the parts involved. He has to contend with neuromata, with adhesions, cicatrices and retracted ends. To secure union he frequently has to resect considerable nerve trunk, for the smallest amount of scar tissue in or between the ends will prevent the penetration of neurones to complete continuity, segment to segment.

To bridge the nerve loss, various expedients are of service. Interposition of arterial segments, segments of nerves from other animals, hollow decalcified bones, and cat-gut bundles have in the past been indifferently successful—in instances part of the shaft of the humerus has been resected to allow approximation of the nerve ends. Best results follow repair by the flap method, either one or both segments sacrificing still more of their structure to close in the gap. At times, it seems feasible to graft the proximal end into a neighboring trunk. This procedure of course damages the integrity of a wholly sound nerve.

In short, though the musculo-spiral nerve can be repaired after a lapse of months or even years, and it is probable that function will be restored to the paralyzed areas, it may again be months or years before the result desired is apparent. Granted aseptic union is secured—and success is impossible without it—with continuity of axis cylinders from central cells to end plates, the proximal fibers, though regenerated, are imperfect, needing the influence of central control to gain anatomic perfection, rendering it possible for them to functionate in normal physiologic way. The central cells do not easily and rapidly yield this influence, their energy being partially deadened—in direct ratio to time since injury—as referred to in preceding paragraph.

Inasmuch, therefore, as the pathological situation becoming worse with the passage of time following the nerve lesion renders operative repair not positive of excellent results, it is essential that the surgeon should secure immediate end to end approximation of axones which have had their continuity interfered with by division, crushing or laceration. Clinical cases have repeatedly demonstrated that early anastomoses bring rapid and certain restoration of function. The proximal segments have retained their potency, and exert marked influence on the development of the new axis cylinders from the neurilemma in the distal segments. Sensation is often present within six weeks and motion in ten or twelve. And rarely is an individual able to work before this

period in an uncomplicated fracture of the shaft of the humerus.

It is not alone incumbent on the surgeon to cut down at once on fractures presenting immediate musculo-spiral paralysis, but his duty extends to the prevention of such disasters as sequelae of bone union. The accomplishment of this is a very simple matter—during the replacement and securing of the bone ends through open incision the nerve may be partially lifted from the groove and surrounded opposite the area where the callus is to form by flaps taken from adjacent fascia or muscle. Such procedure does not allow the callus to grow into, surround or compress the nerve, and its integrity remains undisturbed. Other means of protection are found in the use of sterile gelatin tubes, blood vessels of animals, Cargile membrane, and wax tapes. These are poor substitutes for fascia or muscle.

G. B., aged 67, laborer S. P. Co.—Struck by an engine.

Examination. (a) Simple fracture of right humerus—junction of upper and middle 1-3. (b) Musculo-spiral paralysis—motion and sensation.

Operation. (a) One week after injury—when swelling had receded. (b) Fracture ends replaced and wired—staples were not available—plaster cast. (c) Musculo-spiral nerve surrounded by flap of adjacent muscle.

Pathology. Distal fragment of humerus—overriding proximal to inner side, carrying with it musculo-spiral nerve, which was actually angulated on fragment—perineurium was not broken.

Later history. (a) Sensation present within 3 weeks—complete in six. (b) Motion returned slowly—not altogether normal at end of four months at which time patient passed from observation.

In this case the neurones were severed or crushed without wounding of the nerve trunk envelope. There was no necessity for suturing because of the intact perineurium and the most favorable conditions were present for union of axis cylinder, the cement substance coming wholly from the neuroglia or neurilemma. No interposition of connective tissue was possible. In this connection—when suturing divided nerves, the suture material should never be passed through nerve substance, but through the perineurium. This avoids the possibility of connective tissue proliferation about the axones to jeopardize restoration of function. Had this case been handled by external manipulation and splinting and the nerve condition given over to the mercies of watchful expectancy, it can readily be seen that the manipulations might have cut the membrane which held the segments together, and that one or both would likely have been imbedded in callus or adhesions.

B. E., aged 60, carpenter, extremely muscular. Heavy beam fell, striking arm.

Examination. (a) Simple fracture of right humerus—junction of middle and lower 1-3. (b) Impaired sensation, back of forearm—inability to extend fingers—paresis of wrist.

Operation. (a) Five days later. (b) Fractured ends replaced and wired—great difficulty in replacing ends, which overrode 2 inches—plaster cast. (c) Musculo-spiral nerve surrounded by flap of muscle.

Pathology. Musculo-spiral nerve was partially out of canal, somewhat stretched by distal fragment.

Later history. (a) Sensation completely restored in five weeks. (b) Patient able to resume work in four months—complete restoration of function.

This case presented the symptoms of a stretching of the nerve as evidenced by a paresis of motion and sensation. In such condition there is disappearance of some axis cylinders for considerable distance below the point of stretching. This type of nerve injury recovers of course without operative attention, yet the pathology as demonstrated by the incision made it apparent that the callus in forming undoubtedly would have involved the nerve to its serious damage.

Injuries similar to those of the musculo-spiral occur not infrequently to the ulnar and median nerves when the humerus is fractured near to these trunks. The treatment should differ in no wise from that accorded the musculo-spiral.

Discussion.

Dr. O. D. Hamlin, Oakland: I was interested in Dr. Brown's suture of the musculo-spiral nerve. I have had some of these cases and I must say that my results have not been as successful with regard to the return of the function of the nerve. Dr. Brown states that in 6 or 8 weeks the nerve function is returned. It has not been so in my cases. In one case particularly the function did not return for nearly nine months and I had almost lost hope of any return. This case of mine was not a case of musculo-spiral but was a case of stab wound under the arm where the median and ulna nerve and brachial artery were all severed. Immediate operation was not done on account of the circulation of the arm. It was a question as to whether the circulation would return and as to whether the arm should be amputated. The final result was good, however, and both nerves were sutured about six weeks after the injury and the thing which was the worst part was the retraction of the proximal end of the nerve. The distal end does not retract but the proximal end does retract. We had great difficulty in bringing them together, I used a piece of gauze and opening well up into the axillary space suturing the sheath of the nerve to the muscle because the traction was so great if left to itself the nerve would not hold and the suture to the muscle fixed it and helped to hold it. At the end of four months sensation and motion returned slowly and at the end of nine months the function of the arm was completely restored. With regard to injuries to the musculo-spiral nerve I have seen simple cases of fracture of the humerus at the end have a wrist drop because of injury to the nerve but two cases which I brought had typical wrist drop after about two months, final result was all right, function returned. The open method of fractures is indicated where you have a pathological condition to deal with but I do not believe we can say that every case should be open; I believe we have to study and watch. I do not know of any rule; it is a question of good judgment. I do not believe we should open all cases of fracture of the humerus where the musculo-spiral nerve is probably injured. The doctor thinks that a later operation is not so successful.

NOTICE!

The Register and Directory is now being compiled for the edition of 1910. We expect to issue it in August or September. Please help us to make it as accurate as possible by sending in NOW, any changes of address that have come to your attention. Don't wait a month or two and then send them in; send them in right away.

A CASE OF THEOCIN POISONING.

By E. SCHMOLL, M. D., San Francisco.

The quick and reliable action of theocin has given it a preponderant place in the treatment of cardio-renal disease. Numerous publications attest its efficiency in desperate cases, and enormous diureses are common after its exhibition. These enthusiastic reports induce one to believe that the ideal diuretic has been discovered, and that the older drugs are of use only in the exceptional instances where theocin cannot be given.

The ideal diuretic is a substance which will promptly give therapeutic results without deleterious action on the kidneys or any other organ of the body. Too much cumulative action is also undesirable.

Even a moderate experience with theocin will show that it falls short of this ideal as the by-effects on the stomach and nervous system often prohibit its use and in a few reported cases, death has been hastened.

As caffeine, theobromin, its derivatives diuretin and theocin have practically replaced all other diuretics on account of their recognized efficiency and prompt action, it will be of interest to consider their source, their common characteristics, their several advantages and disadvantages.

All of them are derivatives of xanthin; the hydrogen atom in the 1, 3 and 7 positions having been replaced by the methyl radical. Substitution of one hydrogen yields monomethylxanthin, of two dimethylxanthin, of three trimethylxanthin (better known as caffeine). If in the dimethylxanthin the hydrogen is replaced in the 1st and 3rd positions we deal with theocin; in the 3rd and 7th with theobromin.

Animal experiment and clinical experience have shown that the common and most prominent pharmacological property of these derivatives in moderate doses is a stimulation of the nervous system, the striated muscles and the protoplasm of certain organs, especially the kidney. Large doses paralyze the same organs.

The action on the central nervous system is most marked with caffeine, less with theocin, and practically nil with theobromin. The resulting nervousness, restlessness and insomnia are so severe, sometimes even after small doses of caffeine, as to greatly limit its usefulness as a diuretic. Tremor and twitching of the hands may occasionally be seen. Theobromin and diuretin have almost no effect on the nervous system in clinical doses. Theocin generally shows little nervous disturbance until the third or fourth day. Then the irritability of the cortical motor centers is apparent and may occasionally be severe enough to cause convulsions. In experiments published by Allard, all the animals died in tetanic convulsions. Schlesinger was the first to describe similar results after the therapeutic use of theocin.

A typical case has lately come under my observation which was rather puzzling at the outset. As others may have had similar experiences the report may be of interest.

Mr. J.—54 years old, has suffered for about 2 years from shortness of breath, epigastric oppression and edema of the legs. The first examination showed a dyspnea of 40-50 respirations per minute. The face and arms were very cyanotic and legs, scrotum and abdominal wall edematous. A diagnosis was made of general arteriosclerosis and chronic nephritis with aortic, mitral and tricuspid insufficiency. The liver was very much enlarged, extending 6 cm. below costal margin, and was pulsating.

The treatment is shown in the following table:

Date.	Pulse.	Resp.	Liquid ingested.	Urine.
May.				
13	110	42	800	680
14	112	46	800	960
15	106	36	800	960
16	106	32	800	4800
17	102	24	800	5200
18	106	18	800	5300
19	88	16	750	1560
20	104	22	900	800
21	84	20	900	1230
22	92	22	900	?
22	100	22	690	600
24	88	20	990	1140
25	96	18	900	1260
26	84	20	900	1520
27	84	22	390	2070
28	86	20	600	450
29	88	20	900	900
Medication.		Remarks.		
		Feeling better, not so much dyspnoea.		
3x.1 Fol. digit.				
3x.3 Theocin				
2x.1 Fol. digit.		Feels sick to his stomach.		
3x.3 Theocin.				
Medication stopped.		General convulsions.		
		Shortness of breath, restless.		
3x.1 Fol. digit.		Very restless, nauseated.		
3x.3 Theocin.				
Digit. and Theocin stopped.				
Calomel and Jal. 5 grs.		Nauseated.		
3x.1 Fol. digit.		Very poor night.		
2x.3 Theocin.				
3x.1 Fol. digit.		Nauseated.		
2x.3 Theocin.				
3x.1 Fol. digit.		Good night.		
2x.3 Theocin.		Slept well.		
3x.1 Fol. digit.		Good night.		
2x.3 Theocin.				
3x.1 Fol. digit.		Headache, nauseated, vomited, slept very little.		
2x.3 Theocin.				
Cal. and Jalap.				
Medication stopped.		General convulsions, vomited just before spell occurred.		
Medication stopped.				

The amount of fluid in the dietary was restricted and the administration of 0.3 gm. powdered leaves of digitalis and 0.9 gm. of theocin per day was followed on the fourth day by a tremendous diuresis. For three days the patient passed a daily average of 5,000 cc. of urine, and as he took in only 800 cc. of fluid in the form of milk, he lost his edema at the rate of 4,200 cc. a day, or about 26 pounds in the three days.

On the morning of the second day symptoms of intolerance to theocin were noticed. The patient was nauseated and very uneasy, although his circulatory condition had improved to a marked degree. His dyspnea had diminished, the heart action was better and the edema was disappearing very rapidly. The next morning the symptoms of intolerance to theocin were so marked that the medication was stopped. During the day the nausea grew less but was still marked in the evening. At that time the patient was very nervous, excitable and showed considerable tremor. During the night his wife awoke to find him in clonic convulsions, which, according to her description, much resembled epilepsy. In a few minutes the muscles were set in a tonic contraction and he became comatose and cyanotic, with deep, slow and noisy respiration. As soon as the tongue, which had been aspirated, was pulled forward, the cyanosis disappeared and he promptly recovered.

As this attack coincided with the beginning of an enormous diuresis—nearly 16,000 cc. being passed in three days—the attack was ascribed to a cerebral anemia caused by the sudden relaxation of external pressure on the peripheral blood vessels, and a bleeding into his own veins. Two days later his condition grew worse again, dyspnea and restlessness returned, and the administration of digitalis and theocin was resumed. After 24 hours the recurrence of nausea forced us to give up the medication. Calomel and jalap were given on the 23rd, after which digitalis and theocin were resumed, and continued until the 28th. On the 26th the symptoms of intolerance began to appear—nausea, headache and restlessness—and on the 27th vomiting set in and forced us to discontinue the medication on the next day. On the night of the 28th an attack occurred similar to that of the 18th, excepting that it was milder. The convulsions lasted but a very few minutes and the patient soon recovered.

As on this occasion the diuresis had not been excessive the previous explanation could not hold good. Remembering the publications of Schlesinger and Allard, the convulsions were considered as due to theocin, which was thereafter excluded from his treatment. The patient regained full compensation by June and remained well for three months before he began to fail again. The physician in charge, in whose absence I had treated the patient, and whom I had forgotten to inform about the toxic effects of theocin, gave him the same combination of digitalis and theocin. Five days later, nausea, restlessness and vomiting recurred, so that the medication had to be stopped on the sixth day. In the evening of that day the clonic-tonic convulsions appeared in the same form as on previous occasions. The patient recovered and is at the present time fully compensated.

Besides the publications of Schlesinger already mentioned, there have been reports of similar cases from Strass, Hundt, Jacobson and Allard, who besides his two cases and some very conclusive animal experiments, gives an excellent review of the literature.

Schmiedeberg, after reviewing these nine cases, denies any direct connection between theocin and the epileptiform convulsions for the following reasons:

Firstly: The convulsions occurred in most of the

cases one to two days after the medication had been stopped, and in only a few did they occur during the use of the drug.

Secondly: The convulsions which in the animal experiments are produced by theocin are tetanic, while in all of the reported cases the convulsions were of a clonic-tonic type.

Thirdly: The convulsions may be of uremic origin, produced by the sudden mobilization of waste products due to the resorption of edema following the use of theocin.

It is only fair to say that these objections may be raised in regard to some of the cases, but do not apply to case 1 of Schlesinger, the two cases of Al-lard, and case 2 of Jacobson.

Schmiedeberg's several objections may be answered by the following arguments:

Firstly: In all these cases the toxic symptoms increased in severity with each day elapsed after beginning the treatment, and the convulsions were preceded by nausea, vomiting and nervousness. In the case reported above, the presence of theocin in the body was sufficiently evidenced by the persistence of the toxic symptoms all day after the stoppage of the drug. Then came the convulsions during the night. We must remember that in all these cases excretion was greatly handicapped by the renal insufficiency, and it would be far easier for any drug to accumulate in the system.

Secondly: The change from tetanic to epileptiform convulsions in different species is not without analogy. For instance, morphin acts as a sedative on man and like strychnin on cats.

Thirdly: Our case was observed over a period of nearly two years, and no convulsions ever occurred except after the use of theocin. That the convulsions are not due to the mobilization of waste products is shown by the fact that they do not always coincide with increased diuresis. Also that in the second attack in September, the diuresis following the use of diuretin did not lead to convulsions.

The occurrence of these epileptiform attacks due to theocin should be a grave objection to its clinical use, especially when in a number of instances, death has followed their appearance. These accidents occurred in bodies so diseased that it cannot be said that the theocin was the cause of death, but a drug liable to hasten death can be used only when there is vital indication or after other diuretics have failed.

The second series of organs influenced by the methylxanthines are the striated muscles. The ergograph has shown that caffeine has a powerful effect in delaying the appearance of fatigue and the amount of work that can be performed without exhaustion is greatly increased. The heart-muscle is also stimulated to more forceful and frequent contractions. In large doses, however, the methylxanthins cause a permanent shortening of the fibers, identical with rigor mortis. The diastole is shortened and finally there is complete standstill in systole.

Caffein frequently causes an irritability of the heart-muscle shown by partial contractions and extra-systoles and is especially liable to render a rapidly beating heart irregular. In my experience theocin has not this action, but a typical case due to diuretin has recently come under my observation.

Mr. B.—65 years old, consulted me on account of nocturnal asthma. Examination showed: general arteriosclerosis, interstitial nephritis, high blood pressure and hypertrophy of the left ventricle. The pulse was 92; perfectly regular and of good character. A lactovegetarian diet and 4.0 gm. of diuretin daily was prescribed.

Two days later the patient returned with the oppression and dyspnea increased. Examination showed a pulse of 44, which was found to be due to a bigeminal rhythm in which the second beat failed to reach the wrist. Diuretin was immediately stopped, but the condition remained for five days before it slowly passed off.

The methylxanthins act on the circulation not through the heart but mainly through their powerful influence on the vasomotor centers in the medulla, and blood pressure may be restored to normal in cases of shock. Their action on protoplasm is beautifully shown in experiments with plasmodia where their motility is greatly stimulated by a dilute solution of caffeine. The diuretic action of the methylxanthins is very largely due to their direct stimulation of the kidney cells while the increased blood flow, which has been demonstrated by Loewi in plethysmographic experiments, plays only a secondary role.

In a very meritorious work, in which he compares the activity of the different diuretics, Ach arrives at the conclusion that if the quantity of normal urine be put down as one, the value of the theocin diuresis is 6.3; theobromin 3.8—4.3, and caffeine 2.65. The comparatively feeble action of caffeine is due to the fact that its more prominent stimulation of the vasoconstrictors counterbalances the direct action on the kidney cells and the scant material offered to them through narrowed vessels interferes with the development of a full diuresis.

The conclusions arrived at by the pharmacological comparison of the different methylxanthins are fully corroborated by clinical evidence. If a pure diuresis without any action on the circulatory system be desired, theobromin and its derivative diuretin are indicated as they represent the purest diuretic action. Stimulation of the nervous system and cardiac muscle is minimized with their use, while secondary actions are exceptional and then of but short duration. The relatively slow and feeble action of theobromin is a drawback to its clinical use. A diuresis of at most 3,000 to 4,000 cc. occurs usually on the third day, sometimes on the fourth. Diuretin is always inferior to theocin which is usually active within 24 hours, often within six; and causes the largest diuresis known to medicine, reaching to 8,000 or even 10,000 cc. in 24 hours.

Caffein is indicated if the failure of the kidney to excrete is due to vasomotor paralysis. In these cases, the recovery of vasomotor tonus is followed by marked diuresis. Caffein may be given hypodermatically if an immediate action be imperative, or if the stomach be very irritable.

To summarize; all the xanthin diuretics have a stimulating effect on the nervous system, the heart-muscle and the kidneys.

The nervous and cardiac effects are most pronounced with caffeine; the purely diuretic effect with theobromin, and a very intense diuretic action with irritation of the nervous system with theocin.

On account of the toxic properties of theocin, it should be given only in cases where there is immediate danger to life or where other diuretics have failed. A few words may be added as to the method of administering these drugs and their dosage.

Caffein should be given hypodermatically in doses of about 0.2 gm., repeated three or four times a day. Theobromin, which is insoluble, seems to be preferable to its soluble salts, diuretin and agurin. According to our experience it is better tolerated by the stomach and its action is as prompt as with the soluble salts. Its dosage should be 3 gm. to 4 gm. daily; of the soluble salts 4-6 gms.

Theocin is given in doses of 0.3 gm. three times a day. To avoid direct irritation of the stomach it may be given in suppositories or in enemata. This works remarkably well in a majority of cases but sometimes no effect is obtained. Irritation of the rectum has been reported, but has never been observed by the writer. Its administration hypodermatically is often inefficacious and sloughing of the skin has followed its use by hypodermoclysis. Schlesinger and Romberg advise giving the drug on every second day to prevent stomach symptoms, but in the cases in which I tried this method the diuresis was missing as well as the stomach symptoms.

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PAIN AS A SYMPTOM IN SECONDARY SYPHILIS.*

By EDWARD W. TWITCHELL, M. D., Sacramento.

One can not help being struck by the obtuseness of the general public in the matter of the observation of what, to us, are clamorous symptoms. Proof of this are the numerous cases of tertiary or meta-syphilis whose primary or secondary manifestations have entirely escaped the patient. Within the last few years I have happened to see several syphilitics who were roused from their indifference by the single symptom of pain, totally ignoring the symptoms which had preceded it.

For example, Mrs. X., recently married, was previously the picture of health. I am summoned to relieve the excruciating pain in the joints; pains that she regards as rheumatic. She alludes not at all to a very well marked roseola, general over

trunk and limbs. The various gland constellations are swollen, and in every way she presents a typical picture of secondary syphilis, though she has no recollection of a primary sore. The husband has no trouble that she knows of, but on questioning him, I find that he had a chancre a number of years before, although he had regarded himself as cured, having been energetically treated. The customary treatment overcame the roseola in a short while, and what was of more importance to her, the joint pains which were the occasion of her consulting me, and she was soon comfortable in mind and body. Apropos, of this I am minded of an observation of Fournier's, to the effect that women are less crushed by the announcement that they have syphilis, than are men. The man often trembles, pales, and faints when told that his rash is caused by syphilis, where the woman in like situation, takes it with a surprising coolness. I presume that it comes from long practice in suffering the blows of the male. The spirit of Griselda lives in her sisters of to-day.

Another case exhibits strikingly what I am trying to bring out, for the reason that I took quite a complete history before proceeding to the physical examination, and one notices in the patient's own statement of his case, how all of his symptoms are subordinated to the one symptom of pain.

A. B. is aged 24. Previous personal and family history contain nothing noteworthy. Present illness is of 2 or 3 months standing. First noticed chill and fever, accompanied by loss of weight. Had a lump in the right groin which was opened. This gave him little or no trouble. Coughs a good deal, especially in the morning. Severe pain in the head every night. At times this pain is unbearable. Nothing that he has taken has helped him. His back is weak and aching. He has severe sweats at night. His appetite is fair, and he sleeps when his pain will allow him. In appearance he is pale, hollow-cheeked, and in every respect presents a typical picture of rapidly developing tuberculosis. The one symptom which did not fit into the picture, the lump in the groin, he passed over as of small importance.

On stripping him for chest examination, however, he displayed a widespread maculo-papular eruption. "Well," said I, "when did you have your chancre?" "Oh, that was over four months ago." Inguinal, cervical, and epitrochlear glands were much enlarged, and in all ways he was a classical secondary syphilitic, but after his bubo had healed he paid no attention to any of his symptoms, until the pain in the head and eyes drove him to consult a physician. Appropriate treatment brought the usual spectacular relief. Here again pain was the essence of the trouble, in the mind of the patient.

Now, all of us know that pain may be a prominent feature in the secondary period of lues, no textbook fails to mention it, not even the compend, and some, like the treatise of Fournier, devote chapters to the subject. The pains are divided and subdivided into types, subtypes and varieties, and each is minutely discussed, but the fact of the matter is, that secondary syphilis is usually regarded from the standpoint of its skin manifestations, which of course are fascinating in their variety and picturesqueness, and the other symptoms are apt to be relegated to second place, or entirely unplaced. How many men save the specialists think of syphilitic fever? As a rule when we think of secondary syphilis in the abstract, there jumps into the mind

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a picture of a patient with a spotted skin, a sore throat, or some similar surface lesion.

For this reason it seems to me worth while to emphasize in passing, and with the general practitioner chiefly in mind, the dominant role that pain may play in secondary syphilis, viewed from the standpoint of the patient.

As we pass over any accustomed road, features of the landscape, salient enough in themselves, may be unheeded for this reason or that, or for no reason at all, until one day, or two or three days in succession, something calls them sharply to our attention, and we are jarred into a mild astonishment that we have ignored them hitherto. This perhaps has been my experience, and as human experience is rarely unique, and will be repeated in the future, it seemed to me worth while to lay stress on the importance that pain may assume during the course of the second stage of syphilis.

BRIEF NOTES ON SKIN DISEASES IN THE HAWAIIAN TERRITORY—(THE SUN A MODIFYING FACTOR).*

By HARRY E. ALDERSON, M. D., San Francisco.

These few cursory observations were made by the writer during several months' residence in Honolulu (1907) and a later visit (1909), in the course of which time he saw numbers of patients in the various institutions and hospitals and in the private practices of friends. These observations have been corroborated by many letters received from practicing physicians scattered through the various islands of the Hawaiian group. The majority of these physicians, however, are located in or near Honolulu.

The even, balmy climate of Honolulu makes conditions most favorable for all kinds of life from the highest forms down to the bacteria. The sun through its actinic rays undoubtedly has its effect on the skin. These two factors are directly responsible for the manner in which certain dermatoses appear in that clime. That thermal conditions influence more or less all cutaneous eruptions, and very markedly some, cannot be denied. The effect of the extremes of temperature in modifying these phenomena is familiar to all,—the hastening of the appearance of an exanthem by a hot bath is an occurrence of common observation. One effect (and a most important one) of a constant increased temperature with moisture is seen in the ready growth of the various bacteria and other organisms that find epithelial tissues a favorable culture medium. In Honolulu, with her even, warm climate, one would naturally expect this class of cutaneous diseases to be very prevalent,—and such is actually the case, particularly among the poor class. The various grades of staphylococci and impetigo are exceedingly common and often seen in severe form. Among the poor class it is unusual to see an individual without superficial pus lesions somewhere. Ordinary cuts or abrasions, unless thoroughly treated, are almost invariably infected,—and this is not due to dirt alone, because bathing is universally indulged in, and frequently, too. Some of the worst

cases, however, are seen in the dirty individuals who do not take so kindly to water because swimming involves physical exertion. This class comes from the lowest stratum, of course. Besides the ordinary pus infections, scabies and tinea are very common. Naturally, parasites of all kinds thrive.

The prevalence of leprosy is well known. In the Hawaiian Territory there are recorded 764 lepers. Although the unfortunate victims of this disease are seen all over the world, by far the greatest number of cases are found in endemic foci in tropical and semi-tropical climes; for example, India with 97,340, Japan with 40,000, Philippine Islands with 2330, Java 15,000, Indo-China 10,500, United States of Colombia 4152, Argentine Republic 12,000, as against Iceland with only 200, France with 246 and Germany with 28.

The writer saw in Honolulu a surprising number of cases of pompholyx. Whether this condition is due to a bacterium or to some disorder of the sweat glands, or both, the fact remains that the conditions most favorable for its development are present, namely, heat and moisture.

Some dermatoses are modified, not by the heat of the sun but rather by some other effect of its rays. In the Hawaiian Territory senile keratoses, various other preepitheliomatous conditions, basal cell epithelioma and cutaneous neoplasms in general are extremely rare. These conditions are common complications of senility in less sunny climes, and people live to a ripe old age in Hawaii, so it seems reasonable to assume that this scarcity of senile degenerations of the skin is due, in part at least, to the effects of the actinic rays of the sun. Of course, the absence of irritating factors, such as harsh winds, sudden changes in weather, cold and dust, undoubtedly have some influence.

Lupus vulgaris and other cutaneous tuberculides are extremely rare. It cannot be denied that the sun's beneficent rays are a factor here also. In our own California, where so many of the afflicted tuberculous come for relief, cutaneous tuberculosis is rare when compared with its prevalence along the Atlantic seaboard and in places less favored by sunlight throughout the year.

In such a uniformly warm climate as that of Honolulu the skin is naturally more or less hyperemic and moist most of the time. It is due to this state of affairs that the use of the Roentgen ray demands extreme care and vigilance, for under these circumstances radiodermatitis develops very easily. This was proven in a number of unfortunate cases where exposure was not unduly prolonged (as measured by standards elsewhere) and severe burns resulted.

The foregoing, which is intended merely as a brief preliminary report, is based on personal observation of numbers of patients of all kinds and supported by the testimony of physicians from all over the territory, and the following conclusions seem to be justified:

Climatic conditions can greatly modify various diseases involving the skin.

The sun's rays seem to have an inhibitory effect on the development of some dermatoses, notably senile keratoses, precancerous conditions, basal cell epithelioma and other cutaneous neoplasms, and the tuberculides.

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SYMPOSIUM ON THE PROSTATE.

Fortieth Annual Meeting of the Medical Society of the State of California, Sacramento, April 21, 1910.

ACUTE AND CHRONIC PROSTATITIS.

By VICTOR G. VECKI, M. D.

Until a few years ago the anatomy, physiology and pathology of the prostate were not considered of much importance. In 1888 it took quite a while to search for literature on the subject, and when all of any importance was collected, the reading did not take very long. I defy any one to attempt the gathering of all the scientific productions on the subject written since. I defy also any one to condense into a ten minute paper the subject of acute and chronic prostatitis, and you will kindly forbear if I just run over the subject cursorily.

The differences in the anatomical description of the prostatic gland which we find even in textbooks can easily be explained by the almost regular individual differences, and by the almost incredibly frequent pathological changes. Our real difficulties begin when we enter upon the discussion of the functions of the prostate.

Of course the physiology of the prostatic gland is perfectly clear to various excellent physiologists and urologists, the trouble is only that there is such a diametrical divergence of opinions and even of results in the most essential experiments and deductions, so that we must come to the conclusion that some of the things that seem to be so clear to several scientists are not correct.

We must agree with Reynolds when he claims that we are unable to say definitely what the exact function of the prostate is, though it is now generally conceded that the physiological function of the prostate is almost exclusively a genital one. But even Nagel when explaining the functions of the prostate in his handbook of physiology is rather guarded in his statements, just saying that the development of the prostate being characteristically different according to the different ages points distinctly towards its relations to the sexual activity.

We learn by our clinical experience that however things may be, the prostate has an important part in the sexual life and also in the act of micturation. In the latter regard its office may be of no importance under normal circumstances but jumps into great prominence as soon as pathological changes have taken place.

In acute inflammations we are surely tempted to consider the prostate to be a urinary organ, and are not surprised that once the name of orbicularis vel sphincter urethrae for both the prostate and the prolongation around the membranous urethra was proposed.

The part which the prostatic secretion has in regard to the semen is, in spite of numerous inter-

esting and meritorious experiments, not quite clear, and it is rather significant that even the chemical reaction of this secretion, and its importance as to the vitalization of the spermatozoa is not an undisputed fact, although we know that necrospemia is a frequent consequence of prostatitis.

Thus we may understand why the modern urologists feel compelled to return again and again to the study of the prostatic gland. Physicians told me repeatedly that when referring a case to a urologist they are almost invariably told the patient has prostatitis. But we know that prostatitis is the most frequent complication of gonorrhea, that gonorrhea is the most frequent disease, and that prostatitis is most difficult to cure and our fellow practitioners may easily understand and also forgive that the diagnosis of prostatitis is so frequently made.

So long as the gonococcus has not invaded the prostate the gonorrhea is amenable to treatment, once in the prostate the way of invasion of important organs is open by the road of the bladder to the ureters and kidneys, by the road of the so-called vas deferens, which is no vas but a duct, to the epididymis. Wolbarst emphasizes with right that the prostate must be examined in every case of gonorrhea before the patient is discharged and that the condition of the prostate must be ascertained from time to time in the course of the disease, and I am sorry that he does not tell us what he does to nip in the bud time and again "what might have insidiously become a chronic intractable gonorrhea." It was my experience that when the gonococcus once has invaded the prostatic gland, there is no more nipping in the bud to be done by the physician, nature may accomplish it if there is no interference by the patient's misconduct or too strenuous a treatment.

The symptoms are mostly clear in cases of typical acute prostatitis, and then a diagnosis is easily made, one can even go so far as to differentiate between a catarrhalic, a follicular and a parenchymatous form of inflammation, but as a rule the acute stage, the actual invasion of the prostate is not so typical, the symptoms are very light, the patient hardly pays any attention to them, occasionally telling his physician of an increased frequency in urinating, and of a final disagreeable sensation at the end of each micturation; the more or less developed symptoms of tenesmus. And thus, it happens that the first invasion of the prostatic gland is almost regularly overlooked, and so much more regularly because cases of fresh gonorrhea that are properly treated, by carefully observing experts, seldom develop prostatitis.

When the symptoms of an acute inflammation are obvious and violent, principally in cases of follicular or parenchymatous inflammation, even an energetic practitioner may relent and give the afflicted one a chance, in the great majority of the mild cases of acute catarrhalic prostatitis the treatment which did not prevent or probably even caused the invasion of the post urethra is continued, and

the fate of future misery is sealed for a great many men.

With the exception of the not very frequent cases of aseptic prostatitis, the consequence of habitually interrupted coitus or masturbation, prostatitis is always caused by propagation of an inflammation in the urethra.

When we ask how does this involvement happen and what is the immediate agent causing it, how does this agent act and behave? we reach the biggest snag of the whole prostatic question. No doubt the gland is being invaded by a germ. In acute gonorrhea we have the wily gonococcus, and no matter what Albaran may say, it must be the gonococcus that invades the prostatic gland. What happens to the microorganism once located in the gland is at present hard to say. Some think the gonococcus may persist for many years imbedded somewhere in the prostate, and others are of the opinion that the gonococcus makes room to other forms of bacteria.

The question is not so simple as some of our fellow-workers think. The process cannot be tabulated so nicely as Notthafft has done, and I dare to say that the study of this phase of our subject is far from being clear. The task to investigate and to systematize the bacterial genealogy of the prostate may seem very easy, is extremely attractive, but no doubt is worthy of a scientific Sisyphus.

More cases one examines in every direction, more smears one inspects under the microscope, more cultures one makes, more perplexing problems one meets. Dr. Philip Rahtjen was very enthusiastic when I proposed to him that we should go to work and find out all there is to the living germs in the prostate. We were careful to obtain the real and pure prostatic secretion, and in every case several smears were microscopically examined, and at least two cultures were prepared on blood-serum, blood-agar, agar-glycerin or Wertheim medium. While we are at our task since October last, and have fully investigated over twenty cases, we know that we are far from knowing all there is to the living germs in the prostate. It would lead too far to enter into the details of our findings, they will be reported later, but we are even now sure of the following facts: In the search for pathogenic bacteria the microscopical examination of the prostatic fluid is not sufficient, cultures must be made, because a microscopical examination of the smear may show no pathogenic bacteria, and yet after 24 hours we see on the culture media an abundant growth of staphylococci and even gonococci.

The microscopical examination of the smears, mainly, however, the cultures prepared from the secretæ are frequently surprising and very often not at all in conformity with the clinical symptoms. Gonococci, ordinary diplococci and staphylococci often appear to have a peculiar and intimate relationship to each other, and it is quite probable that the medium in which these organisms grow, whether that afforded by the diseased prostate, or any other, exerts a transforming influence and converts one form into another. This theory is so more attractive as it would fully explain the frequent perplexing changes in the clinical symptoms

of chronic prostatitis. We have further found that in the absence of gonococci there appear almost invariably diplococci, morphologically like gonococci, but Gram positive.

In connection with gonococci there are always staphylococci present.

After irritating local treatment the staphylococci and gonococci increase in number considerably.

After injections of autogenous vaccines the number of bacteria regularly decreases, but we never observed their complete disappearance, and it seems as if a prostate once invaded by cocci can never attain its sterility again.

We may easily understand now how Wolbarst could find gonococci in a prostatic secretion after using a deep instillation of a 1% solution of nitrate of silver, though 13 previous examinations showed none, and we can only wonder at the enthusiastic reports of others who perform miracles with stock-vaccines.

Prostatitis is certainly a most serious complication, even optimists must concede that, to say the least, it is difficult to cure, prevention is therefore most worthy of consideration.

All dangerous and unnecessary instrumentation in the inflamed urethra should be avoided, and when absolutely necessary done only in the most careful, thoroughly aseptic and skilful manner. The various playthings, called endoscopes, sold by traveling salesmen, and with which the most skilled could not see a blamed thing, should be thrown in the fire before they are used on the unwary confiding patient *ut aliquid fiat*.

And as at all occasions, I repeat here most emphatically, that any solution which could not be applied to a person's eye has at no time any business in anyone's urethra.

The deep injections or installations of various strong solutions into the posterior urethra directly to the complicated structure of the colliculus seminalis, this baneful inheritance from Lalemand, modified by Ultzman, clinging against all reason to modern urology is certainly responsible for many a case of grave prostatitis, and should be relegated to the ash-heap of old superstitions.

CAUSES AND DIAGNOSIS OF PROSTATIC HYPERTROPHY.

By M. KROTOSZYNER, M. D., San Francisco.

The etiology of senile prostatic hypertrophy was for many years past the object of careful study and investigation. Since the various classifications, based upon the histologic nature of the enlarged gland, did not prove satisfactory to critical etiologic research, it was not surprising that year after year renewed efforts were recorded in the literature, which if they did not solve the problem, at least were apt to throw more light upon this obscure field of investigation. The research work done a few years ago by Ciechanowski (1) which was carried out on a large scale, gave a new impetus to a more concentrated study of the subject. Ciechanowski considers hypertrophy of the gland to be due

to an involvement of its acini from which inhibition of their secreting function results, a condition which, in the great majority of instances, occurs on the basis of a previous gonorrheal infection. Also Bolton-Bangs (2) and Crowell (2a) believe prostatic hypertrophy to be due to an inflammatory process. In Bolton-Bangs' opinion the main cause is an hyperemia due to chronic irritation (masturbation, excessive sexual excitement, etc.). A similar opinion is voiced by Rothschild (3) who from his own investigations concludes prostatic hypertrophy to be the end—result of a chronic prostatitis, which, obviously, in most instances is caused by a previous gonorrheal infection. In direct contrast to these views are the results of Raskai's (4) investigations who found only two hypertrophies among 98 old men with a definite history of a previous gonorrhea, while of 139 men with no gonorrheic antecedents 21 offered the subjective and objective symptoms of prostatic hypertrophy. The etiology of this condition lies, according to this investigator, in the multiplicity of the physiological functions of the gland, which it exerts upon the organism; its hypertrophy is not due to overactivity, hyperemia or inflammation, but is caused by multiple irritaments through which, in the aged, an enlargement of the organs takes place. H. Young (5) in a recent publication, denies any etiologic relation of chronic prostatic or gonorrheal infection to prostatic hypertrophy. Bruni (6) examined over 1000 inmates of the various poorhouses of Naples between the ages of 46 and 97 with a view to obtain some reliable facts upon the etiology of prostatic hypertrophy and arrived at the following conclusions: All generally accepted etiologic factors (sedentary habits, horseback-riding, sexual excesses, alcoholism, urethral stricture, etc.) are negligible quantities from an etiologic standpoint, the real cause of the condition is as yet unknown and particularly the question why hypertrophy generally occurs in advanced years cannot be answered correctly.

It is a strange coincidence that an almost equal opinion was ventured 50 years ago by the late Sir Henry Thompson (7) who in his admirable monograph upon the diseases of the prostate points to the fact, that the best proof of our ignorance as regards the real cause of prostatic hypertrophy is furnished by the manifold etiologic factors which are made responsible for its existence.

In looking over my comparatively small material in order to ascertain the relationship of a prostatic hypertrophy to gonorrhea I am forced to the conclusion, that a previous inflammatory condition of the gland (gonorrheal prostatitis) should be eliminated from the list of causative factors.

About 25 years ago Launois (8), one of Guyon's gifted pupils, published the results of his careful investigations upon the subject. According to this author the enlargement of the gland is simply a local manifestation of an arteriosclerosis which has

invaded the whole genito-urinary tract. The fallaciousness of this view, which at first was generally accepted by the French school, was proven by careful anatomical investigations done by various authors. Especially the work of Casper (9) who found only 9 cases of endarteritis of the bladder vessels among 28 cases of hypertrophied prostates and that of Motz (10) who found the same small number of cases with sclerotic changes in the vessels of the urinary tract among 31 cases of senile prostatic hypertrophy, have proven that hypertrophy and arteriosclerosis are not rarely coincident lesions in old men but that no common etiologic link exists between these two conditions.

From the foregoing brief review of the most important publications on the subject of more recent date we may conclude, that a cause for the etiology of prostatic hypertrophy is still absolutely unknown. Personally I think Casper's (11) views upon this subject as coming nearest to the truth. According to him hypertrophy of the prostate represents an abnormality but no disease, which only becomes a condition of pathologic importance as soon as through the enlargement of the gland certain untoward and distressing symptoms are apparent in the urinary tract.

This leads to the consideration of the diagnosis of the condition. My experience with the symptomatology of prostatic hypertrophy has taught me that all symptoms from the beginning up to the last stages of the malady including its sequels and complications whether they be of a mechanical or infectious nature are dependent upon the impediment to the urinary flow caused by the change in the size or shape of the prostate. Digital examination from the rectum, which many consider one of the most important diagnostic means, has often disappointed me. It is safe to say, that the ball-like shape of the gland and absence of the raphe between both lateral lobes are characteristic of hypertrophy. I have though been surprised to ascertain through rectal palpation enormously enlarged prostates with few or no untoward urinary symptoms and it is well known that a comparatively small gland may be found in the presence of very distressing symptoms. In such cases v. Frisch (12) advises to resort to combined or bimanual palpation. With the patient in the dorsal posture the index of one hand is introduced into the rectum, while the other hand tries to approach the finger by pressure upon the symphysis. In lean individuals and with their abdominal walls relaxed it is possible, by these means, to ascertain the size and shape of the gland. If this maneuver is carried out with the patient's bladder full, one is able from the size of the fluctuating tumor, which is palpable between both hands, to approximately estimate the amount of residual urine. This procedure may occasionally give valuable diagnostic data in cases of the initial stage; for later stages I prefer to ascertain the size and configuration of the gland with the finger in the rectum, while with the other hand a steel-sound or cysto-

scope, the beak of which lies in the prostatic urethra, is gently moved about. The introduction of a steel instrument should, for obvious reasons, be omitted as long as possible and anxious as the modern urologist may be in almost all cases of urinary disturbances to establish a correct diagnosis through the evidence furnished by the cystoscope, he ought to refrain from the use of this diagnostic means in old prostatics of the first stages, whenever there exists a reasonable hope for the betterment of their condition by an expectant and conservative regime.

Whenever though the malady has entered into the latter stages the introduction of a steel instrument and particularly that of the cystoscope is strictly indicated for diagnostic purposes. I consider cystoscopy in old prostatics when performed *lege artis* and on the basis of strict indications to be the most important aid towards a correct interpretation of the subjective and objective symptoms. This method of examination though should always remain in the hands of the well trained and experienced urologist, otherwise more harm than good will result from it. Very often a poorly executed cystoscopy on an old prostatic with insufficient power for bladder-expulsion will be followed by a hemorrhage from the vulnerated prostate, or a cystitis or even an ascending renal infection. It is therefore not advisable to cystoscopize these patients at the office and I insist upon the procedure being carried out, if possible, at the hospital after a careful preparation. It is not in the scope of this paper to dilate upon the cystoscopically characteristic and important findings. In many cases I have found the graphic determination of the different parts of the vesical outlet as indicated by Young of great aid in order to ascertain the exact points at which the impediment to urination was located. The differential diagnosis between arteriosclerosis of the bladder vessels and true prostatic hypertrophy was only possible by cystoscopy in two of my cases. Complications of hypertrophy with vesical stone, tumor, pyelitis and pyonephrosis can, in most instances, only be diagnosed through the aid of the cystoscope. From the intensity of the trabeculosis and the size and number of diverticula as seen cystoscopically, one is enabled before operating to arrive at a fair conclusion upon the post-operative bladder-function.

Schlaginweit's retrograde cystoscope has not proved in my hands as useful from a diagnostic standpoint as it promised at first. I could always ascertain a thoroughly satisfactory view of all parts of the vesical sphincter with the ordinary Nitze instrument. I would advise though on account of the frequent smaller or larger hemorrhage incidental with the introduction of a steel instrument in these cases, to always use the irrigating cystoscope; by these means the sanguinolent bladder fluid can quickly be changed to a clear one and by having the bladder filled gradually while looking through the cystoscope one can obtain a perfect view of the trigone with its ureteral orifices and look into an existing recessus.

Hemorrhage from some point of the urinary tract is a very frequent complication of prostatic hypertrophy. This fact needs to be accentuated because

in the minds of many physicians an hematuria which occurs in a prostatic at once sets up the suspicion of cancer. It is well therefore to recall the statement of such an experienced authority as Guyon (13) that the prostate is the most frequent source of bleeding from genito-urinary tract. Abnormal vascularization or arteriosclerosis are the main causes for this phenomenon. I have observed hematuria in all the three stages of prostatism and in not a few cases my tentative diagnosis of malignancy was fortunately not borne out by the future course of the disease or by its postoperative findings.

This leads to the consideration of the differential diagnosis between simple hypertrophy and malignancy. Everything of recent note on this subject can be found in the various exhaustive publications of Young (14) who lays particular stress upon the palpatory findings characteristic of prostatic cancer, which he has found to be in evidence in 78 of his 111 cases of prostatic carcinoma. Since, according to this author, in one of four cases of prostatic hypertrophy exists malignancy, the necessity of an early diagnosis cannot be over-estimated.

I am fully aware of the fact that I have only superficially touched upon a few and entirely omitted to mention many other important diagnostic points of prostatic hypertrophy. An attempt to cover this large and exhaustive subject in the short space of time at my command appears obvious. If though I have succeeded to give the impetus to an active discussion, I shall feel amply rewarded for my modest efforts.

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PALLIATIVE TREATMENT OF PROSTATIC HYPERTROPHY.

By E. G. McCONNELL, M. D., San Francisco.

In taking up the subject assigned to me I do not deem it advisable to enter into the pathology and etiology of enlargement of the prostate. To hold to the theory of Guyon that it is due to a general arteriosclerosis, or advance the statement of Harrison regarding compensating hypertrophy, or argue the conditions are due to adenomatous or fibroid changes, as all these will no doubt be included in the other papers.

As the catheter enters largely into the handling of all cases of prostatic hypertrophy, I consider it important to call attention to the character of the urethra and the changes that take place in prostatic hypertrophy. Enlargement of the gland takes place in an antero-posterior direction, or more strictly speaking, in a posterior one. On account of the prostate being more or less firmly fixed at the apex the growth is toward the bladder. As a result of the growth upward and inward the vesicle orifice of the urethra is raised so that the prostatic urethra is more or less bent upon itself, the posterior portion forming something of an angle with the anterior one and interfering with the passage of certain catheters. The unequal growth of the lateral lobes at times will deviate the urethra from side to side, or where the growths are comparatively equal you have formed at times the so-called bladder bar.

The patient presents himself usually with a history of sudden retention following some excess or exposure. The retention being relieved, he has probably remained comfortable for many months when another attack has left him with more or less frequent micturition and some pain. Or your case may complain of frequent urination, and especially of the fact that he is disturbed at nights several times on account of having to urinate; or again the patient presents himself with the statement that he has to pass urine every few minutes. Ask him if he empties the bladder and he will assure you that he does, that he is urinating all the time. It is in this class of cases where the distension has been gradual and no infection has taken place from without, that you will find the large amount of residual urine, one case I remember having sixty-four (64) ounces. No matter what the history of the case, I first determine the extent of enlargement of the prostate and the amount of residual urine. After the patient has passed all the urine he can naturally, I place him horizontally upon the table and after carefully cleansing the parts, introduce a sterile silver catheter. With this I am able to determine if there is a stricture of small caliber; the changes in the urethra due to growth of the prostate; the position of the shaft of the instrument relative to the axis of the body, and the distance traversed before the urine flows. After entering the bladder you are able to determine the character of the bladder wall and the presence or absence of stone. With the catheter still in place and the finger in the rectum you are able to map out more carefully the extent and character of the prostatic enlargement. The fluid removed will give you the exact amount of residual urine. With the patient in the horizontal position at no time have I ever experienced any trouble from removing all the urine in the bladder.

This, in an offhand way, seems easy, but at times you may experience considerable difficulty in introducing your catheter. Remember, *above all things* that no force should be applied to a catheter; be gentle, be patient; time should ever enter into an examination of this kind. If you

are in a hurry; if you have an appointment; by all means defer the examination until another time. As gentleness is of paramount importance in examining the urethra, so it is in examining the rectum. Insert the finger slowly and gently, for remember you probably have more or less of an inflamed condition, and undue force will cause your patient considerable pain, and he at once brands you a brute.

If your examination cannot be finished to your satisfaction in one sitting, either on account of obstructions or fear on the part of the patient, be not discouraged, you are dealing with no trivial condition and if necessary make repeated examinations.

You will at times encounter certain cases of so-called hypertrophy with or without residual urine. They are not cases of true hypertrophy, but are due to a chronic prostatitis. They present a large, smooth, spongy mass and as a rule offer no obstruction to the passage of a catheter. Under massage they rapidly diminish in size and all residual urine disappears. It is this class of cases that often starts a man on the road either to success or failure.

All cases of prostatic hypertrophy should be given a trial with the catheter. If they can be taught cleanliness, have a tolerant bladder, can be comfortable and happy and not have to pass a catheter to exceed four times in twenty-four (24) hours, then life for them is well worth living.

The beginning of catheter life is a grave period, and too much time and patience cannot be spent in properly instructing the patient and fitting him with the proper instruments. Do not give him a catheter and turn him loose, for you will do more harm than good. Teach him to be clean; drum it into him, first, last and all the time. Then select his catheter; if a soft rubber one will answer, well and good; but get the right kind: one with a solid end and smooth eye; show him how to wash it with soap and water after use and how to boil, not cook it. If your soft catheter will not go on account of the bend in your prostatic urethra, use a coude or bi-coude, but don't select a cheap one that will break about the third time it is used. Get the best; something like a Mau, Weis or Porges; instructing the patient that the point must be up, and mark the instrument so he can tell where the point is traveling. Rarely you will find cases where the silver catheter must be resorted to, but when you do, you must watch your patient most carefully until he becomes proficient in its use to prevent his wounding the deep urethra, producing false passages, etc.

The complications that arise as a result of prostatic hypertrophy I will not touch upon except in the briefest way. Cystitis, hematuria, stone, etc., you are all familiar with, but the effect on the ureters and kidney I think should be dwelt upon. Owing to the way the ureters enter the bladder, the urine flows from the kidneys more or less unobstructed, but when the bladder becomes dilated, the ureteral openings are compressed resulting in back pressure upon the kidney. When a man enters upon a catheter life, he is indeed fortunate if he cannot pass part of the urine naturally, for then he

is dependent upon the catheter and the bladder is emptied at regular intervals and there is not that overdistension at times, which produces the back pressure in the kidney and the resulting disturbance in blood pressure.

The regular life in this class of cases must be insisted upon regarding exercise, sleep, diet, drink, etc. However, I do not believe where a patient has been more or less accustomed to alcohol all his life that he should be absolutely denied an occasional whisky and water, for the entire withdrawal of stimulants does more harm to his general condition than an occasional drink will do the bladder.

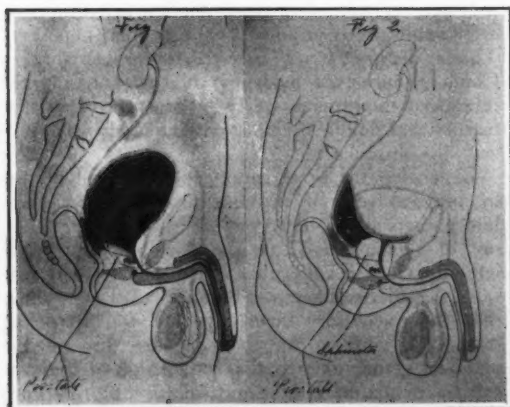
In conclusion I would repeat, give all your prostatic cases a chance with the palliative treatment; if the result is not what you hope, then you can resort to radical procedure, but don't wait too long.

OPERATIVE TREATMENT OF PROSTATIC HYPERTROPHY.

By G. S. PETERKIN, M. D., Seattle, Wash.

I have been asked to present, on Operative Treatment of Prostatic Hypertrophy, an interesting, instructive ten minutes' paper—asked to present such a paper before co-workers, specialists in urologic work. It is indeed a great honor, but it is not with pleasure, but timidity that I accept the task—knowing my inability. Your program committee has, however, made the paper interesting, by setting the time limit.

Enlargement of the prostatic gland, per se, is not the cause of the symptoms of prostatism; it is the extent of obstruction produced to the outlet of the bladder, plus or minus microbic infection. The obstruction is mechanical, so it requires mechanical means to remove it—operation; therefore, operative treatment for prostatic hypertrophy is *not* the radical treatment of this pathologic condition, but the *rational*. That the obstruction is mechanical, you can see, by comparing these two diagrammatic drawings: Fig. 1, shows the position of the bladder



outlet—prostate normal; Fig. 2, prostate enlarged; urethra elongated; outlet, elevated. The bladder contracting, outlet is closed mechanically, by bladder wall; a pool of urine remains the focus for



Figure 3.
A middle lobe enlargement of prostate.

microbic infection—size of pool (amount of retention) depending upon the size and character of the prostatic enlargement.

In the early history of Oophorectomy—ovaries diseased—total enucleation; to-day, we have conservation of ovarian tissue.

All forms of prostatic enlargement are not the same. You have all seen this form of enlargement (Fig. 3). Why not remove this lobe, as you would a bladder tumor; excise the pedicle; close the suprapubic wound; have complete recovery from operation—that is, a well patient, in practically six days; do rational, conservative, prostatic surgery?

In prostatic hypertrophy, as previously stated, we have various forms of enlargement. We have complications—for instance, calculi, sacculations, as diagrammatically illustrated in Fig. 4, drawn from a pathologic specimen, etc. We have conditions simulating prostatism. Four months ago, I was asked to operate for prostatic hypertrophy, a similar diagnosis having been made by five physicians—accepted the diagnosis; saw the patient on the operating table; opened the bladder, suprapubically. My findings, a papilloma the size of a small pigeon's egg, attached by a pedicle, one-half inch long, to the left side of mouth of bladder, anterior to left ureteric opening, acting as a ball valve; prostate, normal. Conditions like this may simulate prostatic hypertrophy, as well as contracture du col, etc., so the surgeon who is going to give not only a correct prognosis and advocate logical after treatment, but also do accurate as well as conservative surgery, on the prostate, must know the exact existing conditions previous to operation. In other words,

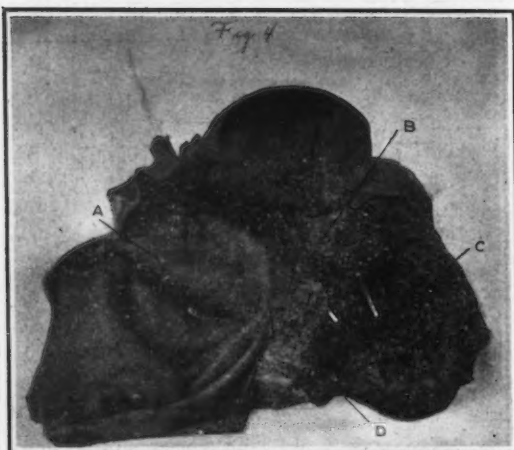


Figure 4.

- A. Center of sacculation.
- B. Opening of same into bladder.
- C. Contracted bladder.
- D. Prostatic urethra.

a thorough cystoscopic examination is indicated in every possible case of prostatism on which the cystoscope can be used.

Experience is the best teacher, as to whether the operation should be suprapubically or perineally performed. If a man have expert teachers and a free clinic to draw from, I might advise him to do perineal prostatectomy; if, however, he has limited experience and desires to give every patient upon whom he operates the best possible results, I advise suprapubic prostatectomy. Suprapubic prostatectomy will give as limited a mortality as does perineal and it will give a larger percentage of complete recoveries—that is, without incontinency, than will perineal prostatectomy in the hands of a man who operates on the prostate infrequently.

Two Points in After Treatment. First: Chronic cystitis itself will cause retention. Most prostatitis operated upon have chronic cystitis. The first stage of chronic cystitis, pathologically, is hypertrophy of the muscular fibers from overwork; the second, sclerosed hypertrophy; the third, atrophy.

This signifies what? That after a successful, either perineal or suprapubic prostatectomy, chronic cystitis having existed, irrigation of the bladder, with a stimulating and antiseptic solution by the patient himself, under hydrostatic pressure, performed psychically, will be of immense benefit, in increasing the bladder capacity and if continued for a considerable length of time, will increase the percentage of complete recoveries.

By psychic irrigation of the bladder, under hydrostatic pressure, I mean that the patient be taught, as every patient can be taught, to permit the fluid to enter his bladder by voluntarily opening the compressor urethrae muscle, instead of forcing it by hydrostatic pressure, beyond this muscle; then irrigation of the bladder is not harmful, but beneficial and will accomplish results aimed at.

Second: After operation, cystitis still existing (it

usually does), use not only psychologic hydrostatic irrigation, but autogenous vaccines, which are beneficial, especially if the infection is due to the bacillus coli.

What has been the bugaboo that has prevented the advance—and it does even to-day—of prostatic surgery, especially prostatectomy, and makes many a man with an enlarged prostate suffer continual pain, be a burden to himself and others? Is it the fear of operation alone or is it the fear of the loss of his sexual power; or that people will think he has lost this power?

One man in this country, Dr. Hugh Young, has made an international reputation for perineal prostatectomy; made it the operation of election. Is it an operation of election, because this operation is less dangerous and difficult to perform, or is it the belief that the ejaculatory ducts are preserved—therefore, the sexual power? It is, in my opinion, this latter belief that the sexual power is retained; and this fact, more than any other, has given perineal prostatectomy its present status; but there is Freyer, with international reputation, who performs only suprapubic prostatectomy and makes a report of 600 cases which ends with these words, "Further, there is no diminution in the sexual power after operation."

What is the true inwardness of all this? Is the sexual power retained or has our knowledge of the part played by the testes in the sexual act been at fault? Let us consider some practical facts of general knowledge: First and foremost, the sexual power is much more psychical than physical. Secondly, daily, we see men who have double epididymitis, whom we know are sterile. Are they impotent? No. Seldom. By disease, the canal of the vas is closed mechanically and the testicular secretion is cut off. Mechanically, by prostatectomy, the testicular secretion may be cut off by closing the ejaculatory ducts. Why should a clean mechanical obstruction cause impotency, as against obstruction produced by disease? In my opinion, it will not. Again, does total ablation of both testes, after adult life is attained, render a man impotent? That it does not is well attested by the fact that, in China, eunuchs are made by ablating not only the testes, but the penis as well; for practical experience teaches that double orchidectomy alone does not prevent the sexual act.

Regarding the preservation of the sexual power, by perineal section, through retaining the ejaculatory ducts intact, let us picture a perfect operation, say, of Young's, and that the ejaculatory ducts are left intact. We know that after prostatectomy, the false capsule collapses and contracts. We know that the ejaculatory ducts are three-fourths of an inch long, running for that distance through the prostatic gland. When this contraction of the capsule takes place, is there any means of preventing the ejaculatory ducts from becoming so tortuous as to close the canal or the fibrous contraction that takes place, from mechanically obliterating their lumen? Who can say it does not? Freyer does not protect them; Goodfellow, who does perineal prostatectomy, does not protect them, and yet, both claim

that the sexual power is retained; but if the lumen of the ejaculatory ducts is obstructed, by either suprapubic or perineal prostatectomy, does obstruction of ducts cause impotency? We know it does not.

Taking into consideration the facts mentioned, is it the psychical belief that he is not impotent, or the fact that the ejaculatory ducts remain patent that causes a patient whose prostate is removed to continue to be potent? Personally, I do not think the patency or lack of patency of the ejaculatory ducts has anything to do with it. The retention of sexual power is due to a psychical suggestion and the man possessed of the belief in himself, in his sexual power, and that it will be retained, will not be impotent, and he who does believe he *will* lose this power, *will*.

This being a fact, it is our duty to teach that prostatectomy will not unsex; it is our duty, for the sake of the patient, so that when this operation is necessitated (suprapubically or perineally), the patient will retain his self respect and not fear that he will be "Oslerized" by either the suprapubic or perineal route; believing in his potency, he will retain power of initiation and be of value in the world's activities.

I repeat, in conclusion, operate for prostatic hypertrophy; operate conservatively, when possible; use a cystoscope before, hydrostatic irrigation and autogenous vaccines afterward; teach that prostatectomy, either suprapubic or perineal, will not unsex.

Discussion.

Dr. J. C. Spencer, San Francisco: I have thought many times when taking part in the meetings of the Pacific Coast Branch of the American Urological Society, that the nub of this situation depends really upon how much was intended for the man doing urological work occasionally. The gentlemen whom we have heard read papers this afternoon have all handled their subjects well. The time has been too limited to cover the ground very thoroughly. I would like to bring out certain facts upon which they have already laid stress, and one is, for the occasional urologist to go slowly. If he is afraid or not sure of his ground, let him keep his hands off. Injudicious medication is so frequently complicatory of conditions which might have been handled differently at the outset, avoiding the distressing symptoms that some enthusiast, not fully cognizant of the possibility of over treatment, may cause, thus complicating the situation. Then the patient is eventually turned adrift or some urologist is asked to see the patient, and finds the situation complicated and the prospect of early relief from the symptoms removed. Dr. Vecki's remarks, that no solution too strong to be used in the eye should be used in the urethra is a piece of concentrated wisdom. I think few of us realize the individual susceptibility of our patients. Some people are intensely intolerant of nitrate of silver solutions. In some they produce a tremendous reaction and irritation and an over sensibility which ties one's hands for future treatment, and the results are unsatisfactory. The points brought out with regard to routine examination of all cases of acute prostatitis are very valuable ones. I think that every man who presumes to treat a gonorrhea is derelict in his duty if he does not go over the individual methodically and also examine his prostatic secretion. Unless one is reasonably skillful with the use of the finger (and I wish here to speak of the comfort to the patient if you will use a rubber finger-cot, well greased, when

making the examination) in securing the prostatic secretion, you will frequently be misled. It is my experience that one is not easily able to demonstrate the micro-organisms in the secretion, as in long continued cases, the infection is liable to be mixed, and one is not able to find all the organisms. So it behooves you to rely upon the culture method. If a man is derelict in his duty and gives his patient a clean bill of health and that individual depends upon this statement, infection is often carried further. Within the last year one of our colleagues in the East accidentally stumbled across the fact that a staphylococcus bacterin used in the case of an individual who had a joint affection, without the gonococcus being demonstrated, (although he was known to have gonorrhea)—that the staphylococcus bacterin cleared up the symptoms immediately. I think that is a valuable hint for future experimentation. Statistics have been mentioned to you, the experiments made by a number of experimenters have been published. It is easy to present statistics, but they depend largely upon the man presenting them and the material from which they are collected. The statistics are too fragmentary and too widely scattered to be wholly convincing. With regard to the treatment of prostatic hypertrophy in a conservative way, Dr. McConnell's recommendations are practically the best that can be followed. In order to spare your patient as much as possible. A most careful painstaking instruction should be given him with regard to the cleanliness of his urethra and bladder and catheter. This will save much trouble. Cleanliness is essential. The comfort which the old man can get from conservative treatment and the sparing him the dangers which may follow operation, are greatly to be desired. In the matter of prostatic hypertrophy, a fact of importance is that the men who are doing the most skillful work and whose results are occasionally published to the medical world, all lay down an axiom, **the necessity for routine cystoscopic examination.** I wish to emphasize the point here that every one is not capable of making cystoscopic examinations. Gentleness is essential, also experience and the ability to translate what one sees. The average practitioner thinks he can insert the cystoscope, but it is not always as easy as it looks. An instance of the difficulties in this work was illustrated in the case reported by Dr. Peterkin of the papilloma in the bladder where a cystoscopic examination would have revealed the condition. With regard to operative treatment, the two men who have done more of this work than all other operators in the world, are Freyer and Young of Baltimore. Freyer is the chief high priest of the suprapubic method, and Young of the perineal operation. Personally I prefer Young's method, leaving the suprapubic route for selected cases. There are individuals who have enlargement of the middle lobe of the prostate in whom all that is necessary is to remove the middle lobe and then the Freyer method is the better. I would like to agree with Peterkin as to the potency in these cases, but the fact remains that these individuals who have had the ejaculatory ducts removed are the individuals who develop impotentia coeundi, and while I do not dispute the statement of Freyer, as to the potency of his operated patients, I would prefer the evidence at first hand from the patients. There are many men whose prostates become hypertrophied early in life but it is a cruel wrong to those requiring operation to deprive them of their potency by an operation, which does not spare the ejaculatory ducts.

Dr. Henry Meyer, San Francisco: I would like to make a few remarks in regard to the diagnosis of prostatic hypertrophy. In regard to the bimanual examination, mentioned by Dr. Krotoszyner, you can never determine with any degree of accuracy the size of the prostate. With the rectal examination alone, you get little information in some cases. Sometimes, per rectal examination, we find no enlargement at all, while on cystoscopic examination we find the prostate extending into the bladder,

causing great obstruction. Again we find large prostates where the patients have no symptoms at all. The best way to get the exact size of the prostate is by measuring the antero-posterior diameter, with the use of the instrument invented by Dr. Francis R. Hagner of Washington, D. C. It has a beak like a cystoscope and depressions in the shaft of the instrument, and with a finger in the rectum you can measure off the depressions. This is a very accurate way of determining its diameter. Again we have prostatic hypertrophy with little increase in the size of the organ, but the prostatic urethra is almost obliterated from pressure upward. That variety is very materially benefited and often cured by complete dilatation of the posterior urethra. In regard to the operative treatment for prostatic hypertrophy, I do not believe any urologists to-day would remove the whole prostate if there was a small lobe with a pedicle, and as far as the different methods are concerned, there is no question but that, while one man prefers the suprapubic and another the perineal, the average urologist who does not have a great number of these cases would do better by selecting the suprapubic route. He sees then pretty much what he is doing and feels everything, the drainage is longer, the flabby bladder regains its muscular power to a great extent and much more perfect than in the average perineal operation. One variety of prostatic hypertrophy, where we find an obstructive band at the neck of the bladder, can be cured by the Bottini incision. This has not been mentioned and most men think it an operation of the past. I do not believe so, particularly in that class of patients where we merely have the band to deal with. If we use the prostatometer of Hagner for measuring the antero-posterior diameter, we know how far it is safe to burn, so that it is impossible to burn through the capsule of the prostate. Also with the control disc invented by Jacobi for the purpose of ascertaining the exact location of the incision, one is absolutely accurate in incising the most prominent situation. This is a most valuable procedure, and it will come into prominence again.

Dr. R. L. Rigdon, San Francisco: There are two or three points upon which I wish to touch, although they have been mentioned already. One statement made on the paper on prostatitis, I believe to be overdrawn. Dr. Vecki makes the claim that when the prostate once becomes infected it is practically never relieved of the infection, and further that the gonococcus is the offending agent in nearly all of these cases. In other words, the man who has once had gonorrheal prostatitis should be forbidden marriage. I believe that the experience of nearly every man who has done a good deal of work in urology is that in a proportion of these cases marriage may be permitted without danger to the wife or the prospective children. No one will go further than I, in discouraging marriage where such advice is necessary, but each case should stand upon its individual merits and not be decided by a general law.

Another point: Dr. McConnell advised that the catheter should be used in all cases of prostatic hypertrophy where possible. I think that we have all come to feel that a dogmatic statement is very apt to miss the mark. To say that all cases of prostatic hypertrophy should be given trial with the catheter, is not stating the matter as it should be stated. Every patient should be investigated very carefully, and some patients be given the benefit of catheter life. The majority of patients that are put upon the use of the catheter, should be so treated until they can be operated upon. I believe that the goal toward which our efforts should be directed should be the removal of the prostate by surgical measures. Another question is what operation should be done. There seems to be a tendency to say that this or that operation should be done regardless of the condition of the patient or the prostate. It seems to me the rational thing to do is to first determine what the general condition of the patient is and then be guided in the selection of our

operation by what we find. In my experience the perineal route as advocated by Young is by far the best in a large number of cases. By this method when properly performed we can absolutely control what we are doing for the whole field is plain before the operator and he can leave or remove whatever he chooses. The functional results are remarkably good.

Dr. C. D. Lockwood, Pasadena: With regard to Dr. McConnell's paper on the conservative treatment of prostatitis, I wish to cite some difficulty which I recently had with a case. Up to two months ago I could say that I never had any serious results from catheterization in determining the amount of urine. The case was a man of 76 with a large amount of residual urine. After he had passed all the urine he could, I catheterized him, but first found that I was unable to pass a soft rubber catheter and then used a Mercier which had been sterilized and withdrew about a pint of residual urine, but still left a large quantity in the bladder. I found upon examination that the bladder was loaded with albumin and urged the patient to go to the hospital where I wished to establish a suprapubic drainage. The patient refused to go to the hospital and I heard nothing further from him for three days. Meanwhile he had called another physician, after having had a chill. He had passed urine which this physician had found contained a large quantity of albumin. I was called in consultation and I went not knowing that this was the same man I had seen. He was in a state of uremia and we did a suprapubic operation and drained the bladder. The man died six days after the first catheterization, from uremia. I believe that the first emptying of that bladder and the neglect for the following three days, precipitated the uremic condition and caused death. I think in the future I shall refuse to take the responsibility unless the patient will consent to go to the hospital. I also wish, in connection with this, to emphasize the importance of drainage prior to prostatectomy. A number of cases which I have seen, I have had where I have first done a suprapubic drainage preceding operation. With regard to the potency in these cases. I will speak from the experience of about 50 prostatectomies. I am convinced that a man is rarely ever potent after his prostate is completely removed and the ejaculatory ducts are closed. I believe that the pleasurable sensations come from the ejaculatory ducts. I believe that spermatozoa and the prostate are essential and I do not believe it is possible to completely remove the prostatic gland and preserve potency.

Dr. G. L. Eaton, San Francisco: I have been very much interested in the papers read here to-day. All of these papers related to prostatic hypertrophy and prostatectomy and I failed to hear any mention made of the after results which we often find after prostatectomy. Of the many men who have done this operation, who will be able to say he has had the same condition existing after that he had previous. I remember one case where the prostate was removed and the canal freed. There was no reason why there should be any residual urine, but there occurred a vesical paresis. We must take into consideration the nerve supply of the prostate. If your bladder has been dilated or contracted by this prostate gland, a certain amount of paresis takes place or an atrophied condition of the muscles, and a great number of these cases are going from bad to worse. With regard to the infected prostate and the use of stock vaccines, I have given some attention recently to the making of the autogenous vaccines made from all the bacteria grown from the prostatic secretion. I thoroughly sterilize the urethra and the glases in which I select the secretion, which I collect by massage. I then inoculate a tube of medium and place it in the incubator and from the different bacteria which grow I make an autogenous vaccine and inject my patient. I have

noticed very remarkable results. Of course, this treatment is at the experimental stage. I do not use a specific autogenous vaccine of one organism, but a vaccine made up of all the bacteria which grow.

Dr. Louis Gross, San Francisco: I have been very much pleased to hear these papers upon this subject. I think that the statement made by Dr. Vecki that we are oftentimes unable to rid the prostate, is well taken. All practitioners have found this to be the case time and time again. A case can be treated for years and still the condition continues. I hope that at some time, and probably in the near future, we will be able to do something with the autogenous vaccine. There are prostates that will not clean up. With regard to acute prostatitis, I desire to mention a case of an individual who had an acute gonorrhea with an acute prostatitis. The prostate was enlarged considerably and there was considerable pus there. No treatment of the posterior urethra availed. The epididymis was involved. I have in some cases, although this one did not show a sufficiently high leukocyte count, done an epididymotomy. I placed this man in the hospital for ten days. The temperature rose and also the leukocyte count. In examination per rectum we found the prostate almost ready to break, and the other physician examining with me wondered how we could get rid of such a prostate. I told him that oftentimes an epididymotomy relieved these cases, and after having done this operation, it was wonderful the way in which the prostate went down. With regard to the use of the rubber catheter, we often have difficulty in introducing it. I have noticed in a recent catalogue a soft rubber catheter bicoudé, which is preferable to the silk bicoudé catheter in the hands of the old prostatic. Regarding the suprapubic and perineal operations, I have seen operators by perineal route who had patients suffering from incontinence for years. There is nothing worse for the specialist than a patient going around with a urinal. For a man who has a limited number of prostatectomies, the suprapubic is the preferable method. There are certain indications for the perineal operation but I think that in 75 per cent or 85 per cent the easiest method is the suprapubic.

Dr. W. P. Willard, San Francisco: I do not think we are justified in drawing any conclusion from one examination or inoculation in infections of the prostate. Dr. Cowden and I have been doing some of this work and we find in every case there are staphylococci present which grow very rapidly and are apt to outgrow all the other bacteria. We might get a slow growing streptococcus, which we may lose entirely if we are not most particular about it. That is also true with the colon bacilli. I do not think we are justified in giving vaccines or drawing conclusions until we have repeated our experiments several times.

Dr. A. J. Zobel, San Francisco: Much has been said about the presence of colon bacilli in the prostate but nothing has been said as to how they gain entrance there, nor has any mention been made as to the methods used. We understand how the gonococcus finds entrance and also the staphylococcus and the streptococcus, but it is through some lesion of the mucous membrane of the rectal wall, which is contiguous to the prostate, that the colon bacilli gain entrance. Ware of New York has found that the bladder is often infected through some lesion of the rectal mucous membrane and it strikes me that the general interest displayed in this line of work should induce an investigation of the rectal mucous membrane before there is an endeavor made to cure the case by autogenous vaccines. It is far better to find the point of entrance of the infection and make some attempt to heal that, than permit the keeping up of the infection after the using of the autogenous vaccines. I would suggest that in the future, where the colon bacillus is the principal bac-

terium found, the lesion of the rectal mucous membrane should be looked for.

Dr. M. Silverberg, San Francisco: In connection with the paper on operative treatment of the prostate, I was rather surprised to hear advocated partial prostatectomy. That is the old McGill operation which in the early treatment of prostatic surgery was found to be useless, inasmuch as it might relieve symptoms for a short while but subsequently the enlargement of the lateral lobes again produced obstruction. In the surgical treatment the gland must be removed as a whole, if it is to be removed at all. In connection with the infecting organisms in prostatitis, I wish to mention a case of proteus infection which I saw. The organism appeared in large numbers in smears made from the prostatic secretion and was subsequently cultivated. The source of the proteus I do not know as yet, but think it very likely from the rectum.

Dr. G. S. Peterkin, Seattle: One point which I would like to bring out is with regard to the after treatment of these cases. The operation is supposed to cure and most surgeons, as soon as the operation is over, leave the patients to themselves. This is wrong for people who have had their prostates removed need looking after for six months or a year. They should be taught to irrigate themselves with warm solution and you will be surprised to find how the bladder will improve under this treatment. If you will do this you will have cases in which there will not be retention of urine. Another point is with regard to using the Bottini. I do not believe it will ever be revived. If the band remains, the simplest thing would be the Young method of punching a hole through the projecting mucous membrane. It is not so apt to cut and do damage as is the Bottini knife. Another point is that the suprapubic route is the simplest one and the least dangerous. Mention has been made that a man should select his cases. A man who operates infrequently is incapable of selecting his cases, therefore the least danger lies in the less complicated suprapubic route. In closing the wound I use only four silk gut sutures, three in the upper part of the wound and one to go through the drainage tube itself. I stitch the peritoneum back so that it will not be infected. Men who operate infrequently cannot safely adopt the preneal operation and moreover the general practitioner in the country has not the proper assistants. The suprapubic operation can be done by one man. In regard to strong injections being used—it is a good rule for the general practitioner not to use them, but there are cases which cannot be relieved except by using strong solutions.

Dr. M. Krotoszyner, San Francisco: The great majority of the gentlemen who discussed the various papers of the symposium confined their remarks mostly to the treatment of prostatic hypertrophy or, to be more precise, to its operative treatment, while very little or nothing was said upon the many interesting points regarding the etiology and diagnosis of the affection. Owing to the shortness of time at my command I was forced to confine my remarks upon the diagnosis of prostatic hypertrophy merely to a few points which in my opinion, are at present in the foreground of attention, viz: the value of the cystoscope as an aid to diagnosis and the differential diagnosis between simple hypertrophy and malignancy. I would have been glad to obtain some of the experience of my confreres assembled here, especially on the latter point. I have spent a great deal of time and effort in collecting and sifting out the most important publications upon the etiology of prostatic hypertrophy which have appeared in the last five or ten years, and in doing so I have found it very hard to give a somewhat comprehensive review upon the subject in the brief space which, of necessity, could be devoted to the subject in this paper. As regards the treatment of prostatitis I only wish to add one word concerning the treatment with autogenous vaccines. I believe that the value of this

treatment is overrated and in the manner in which it is used at present will not hold what it seems to promise. There will very soon be a reaction setting in against the promiscuous use of this method of treatment. The same is true of the operative treatment of prostatic hypertrophy which at present is paramount in the minds of surgeons and urologists to the neglect of more conservative measures which certainly in a good many cases yield excellent results.

Dr. V. G. Vecki, San Francisco: I will say in conclusion that I still stick to my condemnation of the strong solutions. While it is frequently necessary to apply strong solutions to certain parts of the urethra—as might be necessary to apply strong solutions to some lesions of the cornea—it must be done as it would be to the cornea, by treating exactly the point necessary and not flooding the whole tract with such a solution. Another point to emphasize, is that, while I claim that a prostate once infected probably never attains its sterility again, that does not mean that such a prostate remains a source of infection. It certainly could not be attempted to keep all prostatics from marrying. If we had a few of them we might try it; but there are millions, and with these millions we must deal. At the same time we have the clinical experience which teaches that the great majority of prostatics do not infect anyone.

Dr. A. S. Lobingier, Los Angeles: I have been reminded in the remarks made this afternoon, of the notable symposium, at which I was present at the British Medical Association in Manchester in 1902, in which Mr. P. J. Freyer, who had just come over from Dublin, was the first speaker. There were two representatives from America present at the time—Dr. Parker Simms and Dr. Samuel Alexander of New York. Sir William Macewen was present also and led in the discussion of the papers. Dr. Stillman's comments a few minutes ago on Macewen's methods, reminded me of his discussion that afternoon. We know that Mr. Freyer has had perhaps the largest experience of any man living, in this line of work, in connection with his service in the British army in India. He is and always has been a very ardent advocate of the suprapubic method of operating on the prostate gland. If you will remember, at the time there was not so much consideration given to that method of approach. It was not received with very much confidence by many surgeons of the world. The American surgeons had preferred the lower approach and it was mostly in favor among the French and German. I think that we must admit that to-day we are a little more discriminating in our choice of approach to the prostate, being governed by the pathological conditions which we find present. No less a man than Mr. Moynihan has followed Freyer's technic and has advocated it strongly. There are many things concerning the surgery of the prostate and the pathological condition of the bladder at the time of a prostatectomy if senile hypertrophy is present, that enter vitally into the subject and upon which the discussions have scarcely touched. Many of these bladders are either extremely anemic from overdistension and from the infection in the mucosa and submucosa, or they are hyperplastic. The musculature has become entirely changed so that if you were to do an ideal prostatectomy, you would not have ideal conditions remaining in the bladder itself. This leads me to say that the operation of prostatectomy is to a large extent a technical apology. It is practically never an ideal surgical operation. If we are to estimate from the results, we are compelled to consider it chiefly a remedial operation, relieving a condition which cannot be relieved in any other way. I have been privileged to observe something of the results of prostatectomies at the hands of the best masters in the world. If many of these cases are observed in after years, however, we find that not only is there atony of the bladder and residual urine, but there is frequently a stricture at the neck of the bladder, resulting in impaired control of the act of urination. That is another class

of cases which we see not infrequently suffering from incontinence. I am entirely in accord with the idea that the prostate should be approached with critical reference to the pathological condition which we find in each individual case. In many the suprapubic route is ideal. In other cases the perineum seems to be the route of easiest approach. The point made by Dr. Stillman with reference to the technic of these operations—that many cases are not in a condition to withstand a major operation—is important. This is a major operation in the extreme sense. We frequently have a mixed infection, with pus producing bacteria. It is better in such cases to have a preliminary drainage, whether we are to do the suprapubic incision or the perineal. The point should be emphasized that these patients should be in as good condition as possible for the operation which is to come afterward, an ordeal for which not uncommonly they are all too poorly prepared.

THE IMPORTANCE OF MODIFICATION OF THE SENSIBILITY IN THE DIAGNOSIS OF DISEASE IN THE LIGHT OF RECENT NEUROLOGICAL RESEARCH.*

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The paper shows:

I. The fallacies which lurk in the current faulty methods of examining the sensibility and how to avoid them, as for example (a) the stimulation of deep sensibility when the observer believes he is investigating the sense of touch; (b) the fault in technic which allows the patient's perception of attitudes to be complicated by kinesthetic impressions while being examined; (c) the methods of avoiding suggesting symptoms to impressionable patients.

II. The paths in which are grouped sensory impulses in the periphery change their arrangement in the cord. The combinations in the cord are (a) heat, cold and pain of all degrees and kinds, which run in the heterolateral Gower's column, both intermingled and in close proximity to one another; their decussation, however, requiring for completion 2-8 segments, generally 5 or 6; (b) Unconscious afferent impulses of attitude and tonus, both passing in the homolateral dorsal spino-cerebellar and in the hetero-lateral spino-cerebellar tract; (c) Our conscious sensations of the attitude of our limbs and trunk, however, pass in the posterior column; (d) In the posterior column run also for several segments the impulses subserving touch, pressure, and localization. These, however, eventually find their way to and ascend in the anterior columns.

Thus the gray matter is not the seat of any continuous path as was formerly thought. Many of the foregoing conclusions had been already advanced by Brown-Séquard until physiological experiments apparently contradicted them. They have, however, been re-established by more complete and less faulty clinical examinations, which we owe mainly to Head and his associates.

* American Journal of Medical Sciences, April, 1909.

III. To these workers we owe the striking discovery that the sensory impulses before they reach the cord pass along the *peripheral nerves* in combinations very different from the foregoing. They are in three groups: (a) Those of deep sensibility, which proceed from bone, tendon, joint and connective tissue in general, as well as from muscle, where they are co-mingled with the motor nerves. They are stimulated by molecular deformation, and this stimulus when intense is painful; (b) Protopathic sensibility, carried by cutaneous nerves, consisting chiefly of the appreciation of extremes of heat and cold, of pain, and of vague touch; (c) The epicritic sense, ascertained by gentle stimulation of the skin by the very finest cotton wool, by temperatures between 22°C and 47°C, by the power of localization, and by Weber's compass test.

IV. Among the many clinical types of sensory disability the most important from the practitioner's point of view is the *hysterical*; for given the present inadequate knowledge on the part of the laity of normal and abnormal sensory distribution, it is always possible for the expert to detect this form of functional disability by its positive characters, which far exceed in importance any presumptions derived from the history of the disease or the so-called neuroticism of the patient, this latter being indeed often an index of organic perturbation of the nervous system. Where it is a question of hemianaesthesia due to intracranial lesion, the differential characters are the following:

(a) The hysterical anaesthesiae of the present day (1) reach the mid-line exactly, (2) are absolute, (3) as a rule affect all segments equally. (b) When palsy is present, anaesthesia of the affected limb is no help. Variability is rather in favor of organic change than of hysterical fixed idea. The summation of stimuli rarely fails to leap the barrier of an organically interrupted nerve path. Distribution in spots is more significant of tabes or other radicular affection than of hysteria. The implication of all the special senses, however, is strongly presumptive of hysteria, although this diagnosis by no means negatives the organic origin of one or several symptoms present, which are very likely to be the suggestive force at the root of the rest of the syndrome. (c) It is to the demonstration of its induction by suggestion, however, that we must refer the criterion of the hysterical nature of any anaesthesia. The examiner, knowing from the positive characters ascertained that the anaesthesia before him cannot be organic, then seeks the suggestion at the root of the perturbation. This may be done (1) by a simple anamnesis, (2) by psycho-analysis, either by the association method or during hypnosis or (3) by the effects of therapeutic suggestion.

V. Another important clinical type of sensory disability is the radicular. In this, the distribution

of the anaesthesia, hyperaesthesia, or dysaesthesia is in an area corresponding with the spinal segments or radicular nerves. For its diagnosis it is essential to know the cutaneous area belonging to each of these and also the amount and nature of overlapping of contiguous radicular-nerve distribution. The epicritic elements of these overlap very widely, while the protopathic fibres do so much less. As this distinction is the opposite of that obtaining in peripheral nerves in the mass, it is easy to understand its diagnostic importance.

Sciatica is often radicular in causation; and when so, may be distinguished by the presence of the posterior primary division symptoms, the occurrence of pain upon sneezing or violent coughing, and the relative absence of the sign of Lesègue. It is important, however, to eliminate referred pain. This is done by its distribution; and by the drawing up of the opposite leg when a truly sciatic pain is investigated. The pains accompanying locomotor ataxia are of radicular origin.

VI. The dysaesthesiae occurring in syringomyelia are too well known to demand space here; but those of the Brown-Séquard type were ill understood until studied with the greater precision made possible by Head. He has definitely shown the importance of distinguishing between remote and local interruptions in nerve paths by unilateral lesions of the spinal cord and he has demonstrated how differently are grouped deep touch and cutaneous touch in cord and peripheral nerve.

VII. A clinician who has not a knowledge of the distribution of referred pains is sadly handicapped as a diagnostician. The laws upon which this depends are referred by Mackenzie to the autonomic nervous system of Langley. The most familiar example of its influence is found in the arm pains of angina pectoris. In many respects, the automatic areas correspond to the distribution of protopathic nerves, and Head believes the systems to be identical. There are reasons against this, however; but for these the reader is referred to the writer's article in "the American Journal" and to the writings of Mackenzie. It is possible that the "anguish neuroses" may be due to perturbations of the automatic nervous system; and as this condition is often mistakenly diagnosed as hysteria, clinicians might learn much by investigating corresponding cutaneous areas while the patient is in an acute paroxysm of "angoisse"; for as Lebar has shown, it is only during the acute phase of irritation that modifications of cutaneous areas may be found. The further knowledge of these points may solve many of the problems concerning the pathogenesis of certain insanities and bring further categories within our understanding as organic diseases of the nervous system, as secondary manifestations of disease in other organs, thus once more unifying psychiatry with general pathology.

DR. CAMILLUS BUSH.

Memorial Exercises Held by the California Academy of Medicine, June 28, 1910.

"We are such stuff
As dreams are made on, and our little life
Is rounded with a sleep."

The task before me is undertaken in the belief that it is worthy of any man to speak the truth, never so briefly, of the friend who, being summoned, leaves a wholesome memory as a legacy. Mingled sentiments of sorrow and appreciation have impelled me to speak of a fellow-worker whom I valued more and loved better than I knew, whose brief career is, to me and to us all, a pleasant memory.

Of the man of ideals and achievement, who has fallen under the burden of years, we are wont to say: Your work well done, your task complete; sleep under a laurel wreath, rest; resignation is ours, and the world moves on.

But on this occasion, we recall our friend "whose eyes saw only the beauty and promise of Spring; to whom was denied the splendor of Summer, the glory of Autumn, the uncrowned majesty of Winter." Though his years were brief, there can be no doubt that he saw visions of what was in store for him, undisputed leadership, the highest prize accorded to human endeavor.

Mankind very properly takes small note of that sort of conquest which finds expression in mere acquisition, in material profit, or gratification of selfish ambition. Be it said that achievement, never so conspicuous, when inspired by a spirit of self-seeking finds small place in the annals of an epoch.

On the contrary, tribute of a nobler sort and generous, comes unsought to the man of infinite patience and of self-sacrifice, the man who never tires and never murmurs, the man with that Heaven-born interest in his fellow man which is the genesis and the genius of worthy activities.

Such tribute, we of "The Academy of Medicine" offer to the memory of our beloved and deeply lamented fellow, Camillus Bush.

With no wish to intrude upon the sacred privacy of the grief-stricken, we would respectfully extend our profound sympathy to the devoted parents and the loving sister along whose pathway has fallen a dark shadow which will endure to the end. May they, who knew what Heaven had given to the son and brother, find substantial solace in the assurance that his name and influence will live as an example of righteous manhood.

I speak now for the University of California, his alma mater, which received the boy, clean, earnest, purposeful; and gave to the world the man of keen insight into things, of broad culture, equipped with the essential qualities that make for high efficiency—extreme modesty, power of sustained effort and human interest. Only occasionally is such happy combination of intellectual quality and moral fibre to be found in any student body.

Again, I would speak for Johns Hopkins University. From her he acquired new inspiration and splendid training for a career, full of promise for a life work, that would have laid fresh laurels at the feet of his illustrious Second Mother. She will wait long for fuller justification of the soundness of her academic policy, or more grateful appreciation of her generous gifts.

In behalf of the faculty and student body of the Medical Department of the University of California, I shall speak of Dr. Bush's peculiar fitness for the work of a teacher. I would like you to know him as his comrades and co-workers knew him, for he was a man to have known.

There followed him to his new home traditions of a worthy student life, that gave assurance of moral and intellectual uprightness, essential attributes of perfect manhood.

He came among us unobtrusively, unannounced, asking only for a place as a helpful worker in congenial environment. He was in no sense an opportunist. Opportunity he sought, but to him opportunity was a vision, not a dream. It was the portal to activities—having little to do with the immediate, everything to do with the ultimate. Material success was a circumstance, not a triumph.

A scholar by inclination and discipline, he faced the sterner side of life seriously and always in the attitude of an enquirer. He was intellectually restless. There was no halting in his activities, no suggestion of marking time without advancing. With current progress he was not content; with mediocrity, which, Chesterton tells us, is the inevitable doom of the worshiper of success, he had no patience.

As a teacher he easily won the affection and respect of his pupils. In the laboratory, where he did his best work, his role was that of a fellow student, and he was the best student of them all. His influence was that of a stimulus, and in large measure he possessed the faculty of arousing a lively, enduring interest in things uninteresting. The more abstruse the problem, the greater his interest in its solution. His early association with men of renown gave him a clear conception of the manifold exactions that confront the modern teacher of medicine. It is to be said that the work of teaching is to become a specialty, always with the understanding that the term "specialism" as applied to pedagogy in general possesses a larger significance and is capable of a broader interpretation than attaches to the skilled practitioner or the expert technician. In a word, it will happen in the near future that he who teaches effectively will be something greater than a doctor, greater than a surgeon, greater than a man of science.

This thought seemed to pervade the atmosphere that surrounded Dr. Bush, and he entered upon a teaching career manifestly willing to pay the price, to make the sacrifice. Without doubt this thought was his best incentive to effort during long years of preparation; it was the shrine at which he knelt.

I close what I trust you will accept as a conservative estimate of the scholar of brilliant attainments, of the teacher of promise, whose untimely death leaves an unfilled place in the roster of his University, with a brief reference to his personality.

Always gentle and retiring, self-composure was a distinctive trait. Given to few words, he was reticent almost to diffidence. His keen sense of humor and thorough genuineness won for him that fine appreciation which makes for ideal companionship.

His intimates were impressed by his intellectual honesty. Better than most men, he seemed to know his own limitations and was positive rarely. Many of his utterances were couched in the form of a query which cordially invited response.

The critical faculty he possessed in large measure, and in its exercise he was uncompromising in self-exaction. His own belief, his own impressions, even his own faith, were always in the strongest light.

With his fellows he was generous, his points of view were manifold, and he shunned the common error of confounding dignified criticism with vulgar faultfinding.

And now to you, his friends, his colleagues, his fellow students, his pupils, I entrust the memory of Camillus Bush, firm in the belief that his life, brief as it was, will continue as an influence to make better the lives of those who knew him, and who will know him through the coming years.

He was clearly of those who strive

"To know their own most secret thoughts,
And others' too."

T. W. HUNTINGTON, M. D.

Camillus Bush was born at Woodland, Cal., on the fourth of February, eighteen hundred and seventy-eight, and was hence but thirty-two years of age at the time of his death. He was of English descent, tracing his ancestry through the Nelsons of Virginia and Governor Thomas Nelson of Virginia, whose father was Governor of the Kings Colony, Virginia; also through Captain James Bush of the French and Indian War, and of the Continental army under Washington. His immediate grandfather, Nelson, was a California pioneer, coming to this state in eighteen hundred and forty-nine. His paternal grandfather was Dr. J. P. Bush, who came to this state in eighteen hundred and forty-six, and was well known in this city in the seventies. He was for many years and up to the time of his death in eighteen hundred and eighty, in the Pacific Mail service, running between this port and the Isthmus of Panama. Dr. Benjamin R. Swan of this society knew him well and describes him as a most interesting and able man. There were other medical men in the family and a professional life seemed to come natural to Camillus Bush; his early tendencies and ideas were in that direction.

Dr. Bush received his early education in the public schools of this state. He entered the University of California as an accredited student from the Oakland High School, graduating in eighteen hundred and ninety-eight from the College of Natural Sciences with the degree of Bachelor of Science. His college course, while shaped to prepare him for his subsequent medical training, was a decidedly broad one. He seemed to have a fondness for mathematics, taking for more than the required amount for his work. This tendency apparently led him at some time into astronomy, for I well recollect my surprise recently at the intelligent part he took in a rather technical astronomical discussion. The record of his studies at the University shows that he attained high marks in all of his subjects.

Dr. Bush received his medical education at the Johns Hopkins Medical School, from which he graduated in nineteen hundred and two, standing among the first men in his class. Subsequent to receiving his degree, he served for a year as surgical interne in the Johns Hopkins Hospital and for a second year as assistant resident surgeon in the same institution. During this second year he had ample opportunity to do operative work. In addition he acted during this time as the regular assistant of Dr. Harvey Cushing in his operative work, and it was no doubt this association that stimulated his interest in surgery of the nervous system. I think it was in this branch of surgery that Dr. Bush was particularly interested and in which he would have done his best work.

Dr. Bush returned to San Francisco in nineteen hundred and four and immediately took up the practice of surgery, to which he confined himself exclusively. Of the success he was making in this field and of the future that awaited him it is unnecessary for me to speak; it is well known to all of you.

Soon after his return he joined the teaching force of the medical department of the University of California and continued to serve it faithfully to the time of his death.

Dr. Bush had hardly commenced to contribute to medical literature. I find record of only two papers: one on primary sarcoma of the spleen and one on brain surgery, read before this society some months ago. Both of these demonstrate that he possessed much more than average ability as a writer, and undoubtedly, had he lived, we would have had, in later years, many valuable contributions from his pen.

HERBERT W. ALLEN, M. D.

COOPER COLLEGE SCIENCE CLUB.

Report of a Case of Sick-Headache.

By F. W. BIRTCH, M. D., San Francisco.

This case is only an ordinary, typical, but persistent case of sick-headache. All have undoubtedly seen such cases. Still, for twelve years this patient was incapacitated for her work, leading a life of semi-invalidism. During this time, she passed, unrelieved, through the hands of some of our best medical men. This must have been due to the fact that these men considered sick-headache too ordinary, or too uninteresting for them to consider. For these reasons, together with the fact that the headache could be produced artificially, we thought the case might be interesting to some.

Miss X., a nurse, native of U. S., single, age 25. Father alive and well except for the same type of headache as patient. Mother alive and well. Two sisters dead; one at birth, the other at the age of three years with heart trouble. One brother and one sister alive and well. Grandfather on father's side suffered from headache and stomach trouble, the character of which the patient is ignorant.

Patient had ordinary diseases of childhood, also diphtheria, malaria and empyema. At the age of twelve, she began having sick-headache with vomiting. The evening previous to the attack, she was seized with canine hunger, and would wake the following morning with nausea and vomiting which was immediately followed by severe pains in the head. The patient describes the pain as a huge bolt run through the temples and then screwed up. The attacks came on about four in the morning.

At the age of seventeen, the headaches became more frequent, occurring every Thursday at 4 a. m. Finally, her school work was abandoned and she went to the country for a year. While there, the attacks became less frequent, but on her return to school work, she straightway relapsed into her old condition. When her school life was over, she made a second trip to the country, this time remaining a year and a half. She then entered the Hospital Training School, but soon there was a repetition of the old condition. During the first year, she lost a day about every second week, and many days she worked when she was really unfit for duty. The condition became so bad that she was put to bed in the hospital for four weeks.

The physical examination proved negative except for the stomach. A tender point was found one inch to the left of the median line, just below the border of the ribs; and another was tender to pressure in the tenth space posteriorly just to the left of the median line. This point was also hyperaesthetic to a pin prick.

Laboratory notes were as follows: After Ewald Meal: 70 cc. of well digested food was obtained. Free HCL—33 cc. Total acid 60. No mucus, no occult blood. After Riegel's Meal: 60 cc. obtained, HCL 41. Total 54. No blood, no mucus. Stools showed neg. occult blood at all times.

Notwithstanding the negative laboratory finding, suggestion was made that the patient might still have a gastric ulcer which was not bleeding.

She was put on Lenhardt diet, but with little satisfaction. After a month's vacation in the country, she resumed her work at the hospital, but the headaches were unchanged.

One morning, on a fasting stomach, the stomach contents were aspirated and found to contain: 100 cc. of clear fluid. 27 free HCL. Total 39. Bile present, no occult blood.

Then to see whether or not increased acid had anything to do with the causation of the headaches, we put 6 oz. of HCL into the stomach, the patient being ignorant of the nature of the fluid; the characteristic headache was produced. The stomach was

washed thoroughly with solution of sodium bicarbonate and the condition was relieved.

In conclusion I would emphasize the following:

I. That this headache could be artificially produced.

II. That it has been for one year and a half absolutely controlled by washing with sodium bicarbonate.

III. That taking alkaline by mouth does not abort the attack.

IV. Taking an alkaline by mouth aborts the attack in the case of the father.

Report of a Case of Intra Thoracic Tumor.

By J. B. FRANKENHEIMER, M. D., San Francisco.

The interest in the patient about to be shown centers in the pressure on the superior vena cava with the consequent collateral circulation.

The patient is a male, aged 59 years, a native of the United States and a cigarmaker by occupation. His family history is negative, and there is nothing noteworthy in the previous history except that he denies lues. He uses alcohol moderately, tobacco to excess. For the last 10 years, especially after excitement or exertion, he has had slight pains in his precordium. There has been no palpitation of the heart, shortness of breath or difficulty in swallowing.

Until four weeks ago patient was in very good health. At this time he noticed a swelling on both sides of the neck, which extended gradually and involved the entire face. After two days it partially subsided and then returned. About ten days after the onset of his symptoms he noticed swelling of the upper and lower extremities and that his color was very blue. He has been troubled with cramps in the hands and feet severe enough to cause contraction of the fingers and toes. His voice has become hoarse, but he neither coughs nor expectorates.

On entering the hospital the patient was well developed and nourished. The color of his face and head was dark blue—that of his hands and arms less so, and he was slightly dyspneic. The face and head were very edematous. The eyes were negative. The neck was very much enlarged, especially in the submaxillary regions. The external jugulars were the size of a little finger. The chest and upper extremities, especially the hands and forearms, are very edematous. The former is well formed and both sides move equally. At the insertion of the diaphragm, almost entirely encircling the thorax are many distended superficial veins, the largest being about the size of a small quill, forming a bluish black band. Scattered over the back are many dilated veins. The area cardiac dullness upper third rib, rt. sternal border, sixth interspace in mammary line. Heart sounds at the apex are of equal length, no murmurs are heard, the second pulmonic is accentuated. Over the upper sternum there is an area of dullness 9x7 cm. extending as low as the third rib. There is a diastolic shock indistinctly felt over this area; no murmurs are heard. A slight tracheal tug is present. Posteriorly the dorsal spinous processes are dull on percussion from the first to the fifth. The right pulse is much larger than the left. At the base of both lungs there is dullness with absence of vocal fremitus. The liver is enlarged and palpable, as is also the spleen. Abdomen is otherwise negative. The inguinal glands are few; the epitrochlears not palpable. There is moderate edema of feet and legs. The patellar and plantar reflexes are present and lively. Examination of the urine is negative. The Wasserman reaction is positive. X-ray examination shows a large sharply defined rounded or oval tumor occupying the upper mediastinum.

The main purpose of demonstrating this case is to show the remarkable superficial evidence of the established collateral circulation. On account of the pressure on the superior vena cava and its branch the innominate vein with the consequent stasis in

its branch, the internal mammary, the direction of the blood stream is reversed, and we find the venous blood from the thoracic region reaching the heart by way of the internal mammary veins to the superior epigastric veins to the epigastric and circumplex iliac branches of the iliac veins and so to the inferior vena cava.

As to the pathological diagnosis, we think there can hardly be a doubt but that we have to deal with an aortic aneurysm. The points in favor of this conclusion are the age of the patient, the lack of cachexia, the suddenness of onset with a history of precordial or sternal pain on exertion, indicating involvement of the circulatory organs, particularly the aorta, as there is no evidence of disease of the heart which could cause these symptoms; the positive Wasserman reaction and the sharply defined rounded outline of the mass as shown by the X-ray.

Furthermore the patient has shown great improvement under bed rest and specific treatment; not enough if the tumor were a gumma yet more than if it were a neoplasm. Another point is the perceptible increase in the area of dullness after the patient has moved about.

As for the diagnosis, it would probably be safer to use the broader diagnosis of mediastinal tumor. However, I think it would be justifiable in this case to make a more specific diagnosis of aneurysm of the arch of the aorta. I see no indications of carcinoma. The age of the patient, it is true, is the carcinomatous age, but there is lacking the cachexia, the loss of weight, the enlarged glands, the pain and the cough, which a carcinoma in this situation would cause. Furthermore, the sudden onset with previous symptoms pointing to disease of the circulatory organs, the remarkable improvement with treatment and the fluctuations in area of dullness caused by exercise are all evidence more in favor of aneurysm than carcinoma.

CALIFORNIA ACADEMY OF MEDICINE.

Demonstration of Case of Fracture Treated by Operation.

By TRACY RUSSELL.

I shall demonstrate this patient in order to show the use of screws and plates. The man is 38 years old. Four months ago he fell from a ladder, striking on the left foot, leaving a very bad deformity, according to the X-rays. Two days after the injury I operated on him. The lower articular surface of the tibia was crushed into small fragments. These were removed. The lower end of the shaft was divided into four pieces and impacted between them was the upper portion of the astragalus. I had to cut the tendon Achilles to disengage the astragalus, and by tension and manipulation was able to get the fragments into position. The fracture lines ran up the shaft about 4½ inches. Two of the upper fragments were brought together with a piece of silver wire. The inner malleolus with a portion of the shaft had a tendency to displace inward and could not be retained in position with any kind of material except a plate and screws. I took a piece of this material about four inches long and with four screws, two on the shaft and two on the malleolus, retained the fragment in position perfectly. This was four and one-half months ago. The man was in bed two months, then crutches two weeks, and after that was allowed to bear a little weight on the foot. A brace was fitted about a month ago. I did not see him again until yesterday. In the meanwhile he has been walking with this apparatus on his leg. Yesterday I found the foot rotated slightly outward, so that he was walking on the long plantar ligament. This has produced a little eversion of the foot. I am trying

to get him to walk with his feet perfectly straight and will put a little raise in the shoe or get a Gold-waithe plate made for him. The plate and screws are still on the bone. He is walking now, the little swelling that he still has almost disappears in the morning.

The Immobilization of Fractures.

By S. J. HUNKIN, M. D.

It seems to me that there are three things to be considered in the handling of fractures: (1) That the alignment must be maintained; (2) that the fragments must be in contact and end to end, and (3) that the immobilization must be maintained at all times. It has fallen to my lot many times during the last year, and twice during the past week, to see fractures in which surgical operative procedures had been adopted with good placement of wire and staples, and yet the surgeon, notwithstanding his good operative technic, had failed to obtain immobilization and consequently, union. In one instance the fracture was of the upper third of the femur; in this instance after the placement of the staples, the man was put into bed without any immobilization whatever and lay there several weeks. Later on some bowing was noticed and traction was applied. The fracture occurred about nine months ago and the man is still in bed.

The other case is that of a man who was hurt in a runaway. There was a fracture of the femur at the junction of the middle and upper thirds. It was wired and healed by first intention. A plaster of Paris splint had been applied in such a manner that the upper edge of it extended only 12 cm. above the line of fracture while about nine or ten pounds of the splint extended below the fracture and yet the surgeon wondered why he had not secured union.

Contact between the fragments must be obtained and maintained with some pressure. There is an axiom in surgery regarding immobilization of fractures the essence of which is that, not only the fragments shall be held steadily in contact but also the joint above and the joint below the fracture shall be immobilized. That such an axiom is of the utmost importance there can be no doubt, and yet in the large majority of the fractures which I see this axiom is entirely disregarded. Indeed it is unusual to see a fracture held with the joints above and below absolutely fixed. I am familiar with the work of Champonier who treats fractures without any splint. Concerning his method I have little to say except that the results which he obtains would not be satisfactory to me and would, I believe, in this country lead to malpractice suits. With regard to the part played by splints applied directly to the bone. It does not make any particular difference what the bone is splinted with. I prefer staples for fractures occurring in the vicinity of joints and plates for fractures occurring in the shafts of bones.

Up to the present time I think I have at least twenty-five plates in; of these two have little sinuses which will probably require the removal of the plate later on. I am in the habit of using a plate which is much stronger than the ones shown here and I like to have these plates made for each individual case. I am using much longer plates than I did formerly which allows the screw-holes to be placed farther apart, which is an advantage, and the screws are countersunk. The bone-plate has little purpose, though, except to keep the ends of the fragment in contact; it does not maintain alignment after the first few days, because at the end of this time

the screws are loosened to a greater or less extent; but it does maintain the fragments end to end even if the alignment is lost; obviously the maintaining of fragments end to end accomplishes by far too little because such a condition permits of the fragments being adducted, abducted, flexed, extended and rotated. To obviate this I am in the habit of applying two plates in the fractures of long bones. The plates are applied longitudinally and ninety degrees apart, a procedure which is, mechanically, most efficient because, when so applied, inter-fragmentary motion is reduced to a minimum.

It is also evident that immobilization cannot be best maintained by traction but that traction may be of value as an aid in maintaining immobilization. For the external splint some substance should be used from which a mold of the leg could be made. Dr. Sherman made such a splint from iron-wire which was more efficient than any other I know of, because it was so constructed as to fit the changing contours of the body. It is very difficult to properly pad and accurately apply a wooden splint to fractures of the femur. I am particularly well satisfied with plaster of paris. A splint for a fracture of the femur should extend from the toes to the ribs, the foot being included to prevent rotation of the fragments, and when so applied the joint above and the joint below the fracture are held. Such a splint, together with the bone-plates, allows the patient to be moved about and to be turned over without any danger of disturbing the fragments.

Discussion.

Dr. Stanley Stillman, San Francisco: So far as Dr. Huntington's paper is concerned, I must say that I concur with every statement which he has made. The question whether all recent fractures should be submitted to operation or open treatment seems to me, particularly of late years, to have become analogous to the question of the treatment of an inflamed appendix. The whole medical world is now practically agreed in regard to appendix and gall-bladder treatment. So far as fractures are concerned, Dr. Huntington has stated all the essential points. I do not mean to say that every appendix should be operated upon nor every case of fracture, but where there is some obstacle to reduction or difficulty of retention particularly in a case involving a joint, I regard operation as imperative.

My own attitude in these cases is shown when I say that I state to a patient that I decline to take a fracture case unless I am given the privilege of operating if I see fit to do so. My own experience with operative treatment of recent fractures is less than that of Drs. Huntington and Hunkin, but my experience of later cases has been very considerable. There is a great difference between the ease with which you can restore the anatomical relations in recent fractures as compared with the difficulty you encounter in figuring out the articular or fractured surfaces of fragments which have been out of place for some weeks. When the fragment is partially buried in callus it is a very difficult thing so to remove the callus as to permit of accurate replacement. The time to operate on these cases is shortly after the accident happens; the fragments can be fitted back into place again and the results in fractures involving the joints are excellent provided you get anatomical replacement of parts. If this replacement is not accomplished, the result will be an impaired joint no matter what you do except perhaps in the case of young children where sometimes remarkable function is obtained in spite of

great deformity. I agree with Dr. Huntington again in that more is to be expected of us in the future in the results of the treatment of fractures, and that an X-ray picture of a case after reduction whether operated or not, is of the greatest importance. Dr. Russell's case is a striking example of what immediate operation of fractures will do. If he had treated the case six or eight weeks in the usual way before operating, he would not have gotten this result, nor would any other surgeon have succeeded in getting the result which he has shown here.

So far as Dr. Hunkin's paper is concerned, I do not think that I have anything further to add. I am surprised at his statement that he has found it the rule that physicians in applying splints, leave out the neighboring joints in fractures of the limbs. I know that in my own teaching, I have always taught that in a fracture of the arm, splints should take in both elbow and shoulder; in a fracture of the femur, splints should take in the knee and the pelvis, etc. Dr. Hunkin's statement that no interosseous splint will maintain a fracture in apposition for any length of time without external support, is true and applies to bone as well as muscle, tendon or any other structure. There is no use in attempting to hold two tissues together when there is tension enough to cause the suture material to cut and any interosseous splint should be supplemented by suitable external support.

The great question before us to-night is whether this Academy wishes to proclaim its assent to the proposition that practically all cases of fracture ought to be treated by operation. For my part I think they should, not because it is not possible in most cases to get good results in any case without, but because better results can be gotten with operation. The additional damage done by the surgeon is very little and the risk of infection not great enough with proper technic, to balance the advantage gained by the accurate replacement and retention of the fragments. When, however, the X-ray pictures show that satisfactory reduction has been accomplished, then of course such interference is not called for.

Dr. Rixford: I am perfectly in accord with the views expressed by the last speaker as to the general notions of the operative treatment of fractures. There can be no doubt that if a fracture is sooner or later to be reduced by open incision it is vastly better to operate while the fracture is still recent rather than to operate only after a poor result has been obtained by non-operative methods. The reasons for this are obvious. Shortening or longitudinal displacement is readily overcome; the bone is better nourished not having suffered the atrophy of disuse; the general physical condition of the patient is better; his period of confinement is less and non-union is much less likely to occur than where operation is performed as a secondary procedure. Reduction and retention in splints is still a legitimate treatment, however, and I believe preferable for the majority of fracture cases. It therefore would seem to be important in our first study of fractures to determine whether the given case may be better treated by operative or non-operative methods. In certain groups of cases the decision is not difficult; e. g., the great majority of the fractures of the short bones may be satisfactorily managed without operation; simple transverse fracture of the shafts of the long bones also; but spiral fractures of the shafts of long bones (and this is a much larger class than commonly stated, comprising as it does, most of the oblique fractures) may be best treated by open incision. Spiral fractures are caused by twists and the direction of the spiral is always the same as that of the twist—a right hand twist will produce a right handed spiral fracture (the line of fracture corresponding to the thread of a right handed screw). Since right handed twists are more commonly received by the left leg, spiral fractures of the left leg are most commonly right handed spirals and vice versa, left handed spiral fractures are most com-

mon in the right leg. In these spiral fractures the ends of the fragments are irregular, they have sharp points which cut into and hook into soft parts; they are difficult of adjustment because adjustment to be at all satisfactory must be absolutely perfect; and that is impossible except through an open incision. They present no square shoulders which after reduction will take up the longitudinal pull of the muscles. Comminution is often present. In these and similar fractures if adjustment be made by early operation the difficulties encountered will be a minimum and the pieces will fit perfectly. In late operation rarefying osteitis and deposit of new bone so change the shape of the fragments as to make adjustment uncertain and imperfect. Shortening may be obstinate or irremediable and union in mal-position may necessitate mortising of the bone with permanent shortening.

In my own work I am guided as to the indications for operation very much by X-ray findings—I am almost ready to say that I refuse to take the responsibility of a fracture case unless I can have one or more radiographs taken. The radiograph taken before treatment is instituted is a record which is of value not only in anatomical diagnosis and in estimating the indications for operation and in prognosis but is valuable also for future reference and in forestalling malpractice suits. A radiograph taken after adjustment of the fragments—preferably taken through the splints or plaster dressing—is a splendid check on the efficiency of the procedures adopted.

In my personal work I have had a very considerable experience in the operative treatment of fractures. It has been confined, however, almost entirely to the use of wire and the staples of Arbuthnot Lane which I first saw Dr. Huntington use. With the Parkhill clamp and the Sich and other plates I have had little or no experience. I believe, however, from my observation that the simpler the device the better and that the great majority of fractures requiring operation can be better held by means of simple wires, staples or nails than by these more complicated appliances. The real essential, however, is that the operator shall have a goodly measure of mechanical ingenuity—if he is lacking in that particular he had best not bother with the operative treatment of fractures. As between staples and wire, I believe the wire to be the more generally useful.

This radiograph (plate exhibited) shows a complicated spiral fracture of the lower end of the tibia beautifully adjusted and held in place by one of the Lane Staples. But the radiograph took this other plate at right angles which shows that the staple had swung around on its two ends permitting an antero-posterior displacement great enough to spoil the result. I show the picture as an example of one source of failure of the staple. I may say that a wire judiciously placed, subsequently held the fragments in perfect position.

I often use both staple and wire—wire to hold the fragments in lateral apposition and to prevent rotation and staple to prevent longitudinal displacement. Sometimes it is sufficient to simply put the wire around the bone and in this I believe wire is superior to a metal ferrule but if the bone is strong enough, I believe it is better to pass the wire through drill holes whereby if the holes be properly placed advantage is gained in resisting the forces which tend toward displacement.

It was held not very long ago that a limb once shortened could not be lengthened but it is found that not infrequently a shortening of one, two or even more inches which has existed for a considerable number of weeks may be overcome if you have a good husky man or two or suitable mechanical appliances which will pull out the limb after release of the fractured ends of bone. I have succeeded in this way of overcoming a shortening of the leg of two inches after six weeks, in another, a shortening of one and one-half inches after four months,

and a shortening of a thigh of $2\frac{1}{2}$ inches after four weeks.

As a rule I prefer to operate when the fracture is several days' old—a week or so—and for the following reasons: there is less swelling, less hemorrhage in the operation, less tissue present of doubtful vitality; healing has already begun, granulation cells or round celled infiltration have begun to form in the part by which the danger of infection is greatly lessened; or as an old time practitioner put it "The cells of healing are in the part"; there is time for the healing of abrasions to have occurred; delirium tremens is apt to have been overcome and there is nothing lost either in mechanical conditions looking to the adjustment of the fragments nor in the time of recovery of the patient.

Dr. Rixford exhibited a number of radiographs of fractures of the leg, thigh and arm adjusted through the open incision and held by wires and staples. As an example of the kind of case which is every now and then put up to the surgeon he showed a plate of a humerus fractured in two places $2\frac{1}{2}$ inches apart; the uppermost fragment shattered, and the lowest split into the elbow joint. The uppermost fragment had penetrated the skin and had cut off the ulnar nerve. The condition present was that of non-union of the lower of the two principal fractures, ankylosis of the elbow and complete muscle-spiral paralysis. Operation showed the nerve ends separated about three inches—stretching gained about three-fourths of an inch. Since the restoration of nerve function is paramount and nerves recover much more completely when the freshened ends are in complete contact, it seems best to shorten the humerus $2\frac{1}{2}$ inches or enough to permit of approximation of the trimmed nerve ends without tension. At date of publication (after four weeks) excellent union of the bone has occurred.

Dr. Harry M. Sherman: I think that we ought to emphasize the point made by Dr. Hunkin about the intra bone splint being merely a coaptation suture. What he has said, too, about the inadequacy of splints we all see, especially in cases from the country, plaster of paris being rarely used for these purposes, and even then often inadequately. I believe that all the different methods of holding the bone by the inside splint, the coaptation suture which has been spoken of, should all be at hand when we operate. I always like to have an X-ray picture taken before the operation but I think that each one will agree that when we open a limb we find that the X-ray picture has only told part of the truth, even although taken in two planes, we often will find the uncovered bone shows conditions not represented in the radiograms as Dr. Stillman has found, that in one case we can use one thing and in the other case another, and we have to be familiar with all the methods and to have them at hand. The retention of these things in the leg afterwards, I think, depends entirely upon sepsis and asepsis. I think that something that I learned from Dr. Hunkin is applicable, that is with regard to too much handling of these things which go into the wound to remain. First they are handled by the operation nurses in laying them out on the instrument table and again by the nurse in handing them to you, by your own assistant and by yourself. The things which go into the limbs should never be touched by anybody but the operator himself, he should take them up and put them into the limb, that is what Dr. Hunkin began doing when he began to put in silk sutures in paralytic limbs. There is altogether too much general handling of instruments in our operating rooms. If we can succeed in getting through an operation practically with asepsis (no operation is absolutely so), only such infections going in as can be taken care of by the blood and tissues, almost any substance and any amount will stay. The transitory nature of this work is something we will all agree to. We have had to take out these staples in some instances and they always

come out easily if one pulls them in the direction in which the points lie. I have always used screws, wire and staples except in one instance. In this man (exhibiting radiogram) whose femur was broken just above the condyles in a region where cancellous tissue made it impossible to use a screw or staple efficiently, I put on a wide band reaching from one fragment to the other like a ferrule. The fracture was already an old one when this was done, some callus had been thrown out and this had to be included in the band of ferrule. The band has, however, remained perfectly well in place, holding the alignment accurately; nor does it in any way interfere with function even although it is somewhat near the joint, and much of the callus has disappeared.

Dr. Huntington: I am much pleased at the interest elicited by this subject during the past three years. A comparatively short time has elapsed since frequent resort to the open method in fracture treatment has been seriously entertained. During a recent eastern trip, I was surprised to find how few hospital surgeons manifested a willingness to undertake this line of work. Urgent cases were operated upon without protest, but the idea of approximate reposition seemed to meet the ideals of most surgeons. The question has been asked many times by physicians remote from hospital facilities whether or not they should operate in obdurate cases. The answer is certainly NO.

There is always the alternative of sending such patients to a well equipped hospital, or if that be impossible, the surgeon should protect himself by making a plain statement to the effect that without an operation, a satisfactory result cannot be assured. With a written record of that, the surgeon is safe from subsequent attack. With regard to open fractures, my feeling is that there should be sufficient delay to warrant the presumption that the danger of sepsis is past. We must consider open fractures always as primarily infected and to introduce a new element in the shape of a foreign body under these circumstances is of questionable propriety.

MEDICAL ETHICS IN SAN FRANCISCO.

By W. S. THORNE, M. D., San Francisco.

The following article is a reprint, the original appearing seventeen years ago.

In view of the fact that the subject is one to which attention is constantly drawn by real or fancied transgression of ethical conduct, I venture to hope that the suggestions may not be inappropriate to the present day:

"In California, isolated from the older and more stable societies, the medical profession is characterized by an absence of that *l'esprit de corps* that we observe elsewhere. The explanation of the fact is to be found in the heterogeneous elements comprising the body of the medical profession. The transplanting of men, representing different nationalities, ideals and social conditions, and the consequences that follow the self-restraint and respect imposed upon the individual by the conventionalities of more ancient and crystallized societies, conduce to a diversity of thought and action which we are accustomed to witness here. Provincialism finds expression in self-laudation and an exaggerated idea of the especial superiority of country, educational advantage, and college degree. Nothing so pre-

eminently distinguishes the *small* man—the man whose knowledge of the great world is limited to the confines of his college *campus*, and the geographical boundaries of his native province, as his fancied superiority, and the assumption of great wisdom. Newly released from the pressure and restraint of strict social order, and graded rank, the stranger is prone to regard with ill-suppressed disdain an approach to professional equality with his brethren here. *His country, his attainments, his traditions*, have deeply impressed *him*, and it is only later that he comes to learn, that schools do not make doctors, and that doctors do not make men—that behind the doctor is the *man* and his *character*, which together comprise all that *he is*, and all that he is *worth* to the community in which he lives. The doctor, whatever his attainments, who is dishonorable, and unethical, who maligns his neighbor, who detracts from honest and conscientious effort on the part of an humble confrere, is a contemptible man. The man of many degrees, the man of encyclopediac knowledge may be, and often is, a weak and incompetent practitioner. Colleges may confer degrees, but they do not confer courage, honor and common sense. Men equally educated differ in point of intelligent application of their acquirements. Men unequally educated will yet more widely differ in this regard, but honest and conscientious effort, however ill-directed, is entitled to respectful consideration. Fortuitous circumstances of birth, educational advantages and natural adaptation, place us individually on different planes, but this difference in potential capacity, if supplemented by honest endeavors, should not detract from the respect due to such attributes. The too frequent tendency in San Francisco to utterly ignore the rules of ethical conduct and for one medical neighbor to openly charge another with ignorance or incompetence, is ignoble. The man who indulges in this sort of egotism, is handling a boomerang, quite as likely to injure the wielder as to destroy the object of its aim. In any event, it lessens confidence and respect of the public for the medical profession. The medical function is nothing, if not dignified and respectful. Pope, cardinal, bishop and priest may serve at the same altar. All men can not stand upon the same level, but erudition and superior station should not dull our sense of justice and fair dealing toward the less fortunate. He who imagines himself pregnant with great wisdom should reach the goal of his ambition without injury to the reputation of his neighbor, and without slurring and injurious comment. Let us be *men*, ready at all times to answer for our words and our actions, considerate of the faults and the mistakes of others. No man can rise or has ever risen to an exalted height in medicine who has not carried with him the love and respect of his contemporaries."

SOCIETY REPORTS

RIVERSIDE COUNTY.

The last meeting of the Riverside County Medical Society for the year prior to the summer vacation was held Monday evening, June 13th, at the Hotel Glenwood. The members of the Society entertained their wives, Senator Miguel Estudillo of Riverside, and Honorable George Freeman of Corona, candidate for the Assembly before the Republican Primaries. Senator Estudillo and Mr. Freeman spoke on the subject of "Medical Legislation." Dr. C. Van Zwaluenburg read a paper on "A Model Practice Act."

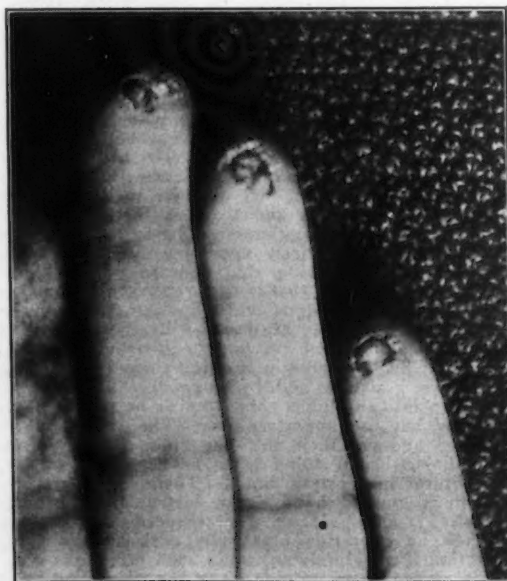
Dr. R. E. Moss of Riverside was elected to membership in the Society.

SAN FRANCISCO COUNTY MEDICAL SOCIETY.

Regular meeting, March 8, 1910.

Dr. H. A. L. Ryfkogel, presenting Case of Tubercular Dactylitis.

Patient, white married woman, 35 years of age, gives no personal or family history of tuberculosis or syphilis. There are no points in the family history that have any bearing on her present condition. Last April she noticed a curious tingling sensation in the left forefinger, which in a few weeks became swollen, and a little later on pus issued from beneath the matrix of the nail. Two or three weeks after this her thumb began to swell and within a couple of months the condition became as you see it now. Examinations of the patient's general physical condition show no disease of any important organ. The radial artery of the diseased side is absent or obliterated. Pulsation of the right radial is normal. Circulation of both hands seems otherwise normal. Examination shows, on the palmar surfaces of the distal phalanges of the third, fourth and fifth fingers, warty growths (see illustration) which rest in sharply punched out excavations. The walls of these snugly grasp the warts. A small amount of pus exudes from between the warts and the walls of the cavities. The second (index finger) is swollen in its distal third and thin pus can be squeezed from beneath the nail matrix. When the



Tubercular Dactylitis.

second joint of the finger is flexed, rough crepitus is felt. The thumb is swollen to twice its normal circumference, the greatest diameter of the swelling being opposite the inter-phalangeal joint. In it crepitus can be felt. This crepitus did not develop until lately, however. There was no considerable redness or pain. The diagnosis here lies between a trophic disturbance such as Raynaud's disease, syphilitic dactylitis and papillomata, and tubercular dactylitis and warts. The tuberculin test showed a slight reaction. A Wasserman test was negative. The swelling of the thumb and finger was opened and some material curetted away and examined microscopically. It resembles syphilitic rather than tubercular granuloma. Finally, guinea pigs were inoculated with some of the material and died of general tuberculosis. Patient has been put on proper hygienic and medicinal treatment, and the usual ascending doses of tuberculin are being administered.

BOOK REVIEWS

Practical Suggestions in Borderland Surgery. By G. M. Blech, M. D. Professional Publishing Co., Philadelphia, 1910.

This book of 200 very small pages is presented, according to the publisher's note as a "veritable *multum in parvo*" a protest against the "somewhat unhealthy stimulation which has resulted from the marvelous surgical successes of recent years." The reviewer fails to understand how the question "when to operate and when not to operate" is solved by very lengthy and laudatory descriptions of Freund's operation for emphysema, Routte's sapheno-peritoneal anastomosis for ascites, the X-ray treatment of prostatic hypertrophy and the injection of alien blood into that gland as a therapeutic measure. It is interesting to read that "the prevalence of pneumonia in the early spring and late fall should cause us to postpone facultative operations to a time when climatic conditions are more favorable." Equally interesting is the statement that the author found it necessary to go to Germany to investigate the essentially American drop method of ether anesthetic and that his experience with cholelithiasis "comprises eleven cases, nine of whom have been operated on, the other two being still under observation (over five years)." Hence the following contra-indications: acute attacks, icterus, occlusion of chole-dochus, obesity, nephritis, etc.! In typhoid perforation the operative chances are termed problematic and internal medication is preferred. Hysterectomy for fibroid is said to have a considerable mortality.

Thus one may scan pages of interminable incongruities woven together without method or grammar.

D. T.

The Elements of the Science of Nutrition. By Graham Lusk, Ph. D. S. D. H. R. S. (Edin.). Publishers, W. B. Saunders Company, Phila.

A second edition of Professor Lusk's work within three years is in itself testimony to the value of his contribution to the most important of all physiological subjects. It is in fact a book belonging to a type too little seen in contemporary scientific literature. The average modern treatise contents itself with stating results, and the larger number of the graduates of our schools have but the haziest notion how these results have been arrived at. Here we have a book whose stated aim is: "To review the scientific substratum upon which rests the knowledge of nutrition both in health and in disease. Throughout no statement has been made without endeavoring to give the proof that it is true." This ideal is strictly

adhered to and we have a volume tracing with the utmost precision and great lucidity of language the long series of experiments and deductions that underlie our present teaching in the field of metabolism. Apart from the knowledge to be acquired, I can conceive of no more valuable mental training for student or physician than is afforded by the perusal and mastery of these pages. Commencing with the fundamental experiments of Lavoissier, the reader is led through the maze of experimentation resulting in the latest achievements concerning the complex problems of proteid metabolism.

Under the headings of Metabolism in Starvation, The Regulation of Temperature, The Influence of Proteid Food, The Specific Dynamic Action of the Foodstuffs, The Influence of the Ingestion of Fat, and Carbohydrate, The Influence of Mechanical Work on Metabolism, A Normal Diet, The Food Requirement During the Period of Growth, Metabolism in Anemia, Gout, Fever, Diabetes, Phosphorus Poisoning, Theories of Metabolism, we find practically all of the leading questions concerning normal and pathologic nutrition considered. The very fairness with which all facts and opinions are presented seems to preclude a full presentation of the author's own opinions, and we miss such able summaries of the drift of opinion as Professor Lusk might well give. This much will be apparent to the careful reader, namely, that however much we know, and though, as the author states, many of the laws of metabolism "are as fixed and definite as are any laws of physics and chemistry," still there is an enormous work to be completed before many of the most pressing questions concerning health and disease can receive a satisfactory solution. Furthermore, it will be seen how slim is the foundation on which rest many of our popular opinions; thus we learn that suboxidation, which has figured so largely in medical literature of the quasi-scientific type, is a myth. While primarily concerned with laboratory experiments and the deduction of general laws, the book is none the less of interest to the physician in the daily practice of his profession, and to such the chapters on Metabolism in Diabetes, Gout, and Fever will have great value. Finally, in the way of criticism we note that the author in his preface states that having collected for the American Text-Book of Physiology, the material for the chapter on the life history of the inorganic constituents of the body, he has allotted but little space to it in this volume. This we feel to be a mistake; to write a work on nutrition without reference to the absolutely essential inorganic elements leaves us with a sense of incompleteness. Lastly, then appears on page 31 the following passage: "Phenomena of life are phenomena of motion due to the liberation of energy in the breaking down of molecules. The motions are principally manifested as heat, mechanical energy and electric currents." Here we find Professor Lusk seemingly giving authority to a widely diffused fallacy, namely, that katabolic processes are a source of energy. Of course the exact opposite is the case, the dissociation of large molecules can only be effected by an expenditure of energy; in other words, the breaking up of a large molecule into many small ones can only be accomplished by work done on it. The liberation of energy that commonly, but not necessarily, accompanies katabolic processes is due to the oxidation of the products, and in the absence of such oxidation is not manifest. At the end of the passage in question the author finishes with the statement, "Metabolism vivifies the energy potential in chemical compounds." As literature this sounds pretty, as a scientific statement it is permissible to ask what it means.

H. D., A. P.

Clinical Treatises on the Pathology and Therapy of Disorders of Metabolism and Nutrition. By Carl von Noorden. Authorized American Edition. Edited and translated under the supervision of Alfred C. Croftan, M. D.

Part VIII. Inanition and Fattening Cures.

Part IX. Technic of Reduction Cures and Gout.

The author's well deserved renown on questions of metabolism is a sufficient recommendation for the above books. They deal with subjects of vital interest to every medical man, for there are very few persons frequenting doctors' offices who do not require so-called "building up," or, on the other hand, advice against overeating qualitatively if not quantitatively.

The entire field of dietetics is one with which but a small proportion of the profession is familiar. There are various reasons for this, the principal one being no doubt that after glancing at various lengthy treatises on metabolism, the reader decides that the problem is a complicated one, and is content to continue feeding his patients in a haphazard way. As a matter of fact, there is hardly any one method of treatment so simple of application, so easy of control, so fruitful in results, as the dietetic. And there is no exposé of the subject so easy to read, so clear, and above all so practical, as the series of monographs by Carl von Noorden. R. B.

Handbook of Therapy. Journal A. M. A., Chicago.

Four hundred twenty-one pages. A compilation from the matter in the Therapeutic Department of the Journal of the American Medical Association, which has appeared during the last two or three years, published by the Journal of the American Medical Association.

This little book is decidedly worth while. It is a pleasing departure from the handbook of the usual type, in that it offers very good abstracts of very good articles which have been written sufficiently recently to present more than a glimpse of the present stage of metamorphosis that the treatment of many very common conditions is undergoing.

It does not deal with abstruse theories nor with untried methods nor does it deal with the treatment of every disease, in short, it does not aim to be a pocket edition of a system of medicine. This has allowed the editor latitude, of which he has very pleasingly taken advantage, to give greater space to matters of interest which may be properly dealt with in a short work, for instance, the six pages devoted to snake poisoning in the United States present very sufficiently a description of venomous snakes in the United States, the symptoms of poisoning, and an outline of practical treatment.

There is also an interesting extract on tobacco. Of the other subjects treated there is found a disposition to select subjects of interest and to deal with them in common sense way. The sketch of the treatment of myocarditis is worth rereading, in fact, it is difficult to pick out a single article that is not of active interest. Lest the reviewer be criticised as being over enthusiastic, it may be said that this is a dangerous book for undergraduates, who, only too easily develop the habit of relying on compendiums, but for physicians who desire to read something to fill in their odd quarter hours this little handbook is found very pleasant.

G. E. E.

PACIFIC ASSOCIATION OF RAILWAY SURGEONS.

The eighth annual meeting of the Pacific Association of Railway Surgeons will take place at the St. Francis Hotel, August 26-27, 1910.

DAVIS MEMORIAL COMMITTEE OF THE A. M. A.

"The first year that your committee was appointed was marked by the unprecedented disaster to the great city of San Francisco, and it was felt that every effort should be made by our profession to relieve its suffering. Although very many wealthy members of our profession in San Francisco were utterly ruined financially, they, for the most part, heroically refused assistance, and the \$5,000 voted by our association for this purpose was refused with thanks. The last contribution received for the Davis memorial was from Dr. R. F. Rooney, member of our committee from California, in the sum of \$130, regretting that this amount was so little, owing to the great calamity which had befallen them."

NEW AND NON-OFFICIAL REMEDIES.

Chinosol (Chinosol Co., Parmele Pharmacal Co.).
Diaspirin (Farbenfabriken of Elberfeld Co.).
Nuclein Solution, Abbott (Abbott Alkaloidal Co.).
Nuclein Tablets, Abbott (Abbott Alkaloidal Co.).
Parathyroid Tablets (Armour & Co.).
Corpus Luteum Desiccated (Armour & Co.).
Pituitary Body Desiccated (Armour & Co.).
Pituitary Tablets (Armour & Co.).
Ferratin (Merck & Co.).
Arsenoferratin (Merck & Co.).
Arsenoferratin Tablets (Merck & Co.).
Arsenoferratosé (Merck & Co.).

NEW MEMBERS.

Rosenberger, H. G., Whittier.
Cunningham, R. L., Los Angeles.
Stephenson, C. C., Los Angeles.
Brown, J. C., Los Angeles.
Smith, S., Los Angeles.
Townsend, V. R., Long Beach.
Martin, M. L., Los Angeles.
Morton, L. B., Pasadena.
Colloran, J. E., Los Angeles.
Remington, L. D., Monrovia.
Clark, C. S., Arroyo Grande.
Alvarez, W. C., San Francisco.
Rude, A. E., San Francisco.
Edwards, Wm., San Francisco.
Bergener, G. J., San Francisco.
Styan, Wm. E., San Francisco.
Yerington, H. H., San Francisco.
Scheier, R. B., San Francisco.
Hicks, J. W., Hammononton, Cal.
Cummings, R. S., National City.
Janes, Manning, La Mesa.
Swearingen, A. Wm., La Jolla.
Hart, Frank, Santa Cruz.

RESIGNED.

Dr. K. I. Howard, 1418 Larkin St., San Francisco.
Kibbe, M. E., Fruitvale.

DEATHS.

Barber, R. D., Corona, Cal.
Burleigh, C. A., Forest Hill, Cal.
McCracken, C. L., Redwood City.
Boxmeyer, C. H., Palo Alto, Cal.
Durst, Daniel, Wheatland.
Baur, R. W., San Francisco.